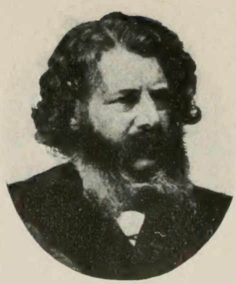

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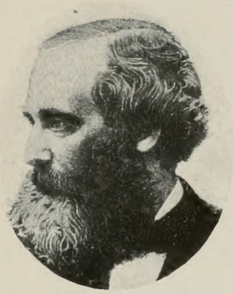

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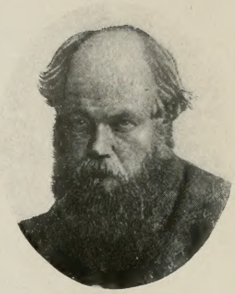

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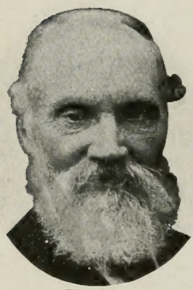

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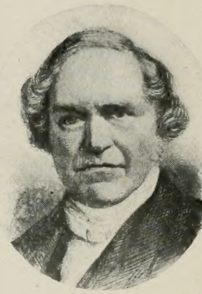

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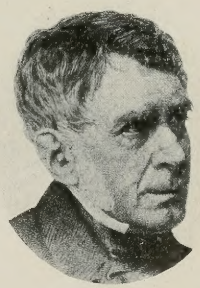

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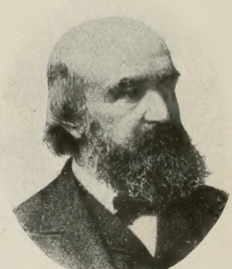

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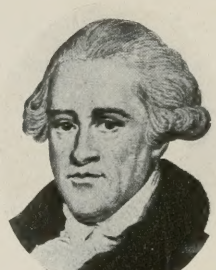

Herschel

MATHEMATICAL MONOGRAPHS

EDITED BY

MANSFIELD MERRIMAN AND ROBERT S. WOODWARD

No. 20

# LECTURES ON
# TEN BRITISH PHYSICISTS
## OF THE NINETEENTH CENTURY

BY

ALEXANDER MACFARLANE

Late President of the International Association
for Promoting the Study of Quaternions

FIRST EDITION



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# PREFACE

During the years 1901–1904 Dr. Alexander Macfarlane delivered, at Lehigh University, lectures on twenty-five British mathematicians of the nineteenth century. The manuscripts of twenty of these lectures were discovered in 1916, three years after the death of their author, to be almost ready for the printer, and ten of them, on ten pure mathematicians, were then published in Monograph No. 17 of this series. Lectures on ten mathematicians whose main work was in physics, astronomy, and engineering are given in this volume.

These lectures were given to audiences composed of students, instructors and townspeople, and each occupied less than an hour in delivery. It should hence not be expected that a lecture can fully treat of all the activities of a mathematician, much less give critical analyses of his work and careful estimates of his influence. It is felt by the editors, however, that the lectures will prove interesting and inspiring to a wide circle of readers who have no acquaintance at first hand with the works of the men who are discussed, while they cannot fail to be of special interest to older readers who have such acquaintance.

It should be borne in mind that expressions such as "now," "recently," "ten years ago," etc., belong to the year when a lecture was delivered. On the first page of each lecture will be found the date of its delivery.

For five of the portraits given in the frontispiece the editors are indebted to the kindness of Dr. David Eugene Smith, of Teachers College, Columbia University. A portrait of Dr. Macfarlane will be found on page 4 of Monograph No. 17.

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# TEN BRITISH PHYSICISTS

## JAMES CLERK MAXWELL *

(1831–1879)

James Clerk Maxwell was born in Edinburgh, Scotland, on the 13th of November, 1831. His father, John Clerk, belonged to the old family of Clerks of Penicuik near Edinburgh, and he added Maxwell to his name, on succeeding as a younger son to the estate of Middlebie in Dumfriesshire, which had for generations been the home of a Maxwell. Hence it was customary in Scotland to speak of the subject of our lecture as Clerk-Maxwell; but by the world at large the "Clerk" has been dropped; for instance the magnetic unit recently defined in his honor is not denominated a "Clerk" or a "Clerk-Maxwell," but simply a "Maxwell." His father was by profession an advocate, that is, a lawyer entitled to plead before the Supreme Court of Scotland; his practice had never been large and at the date mentioned he had retired to live on his estate. John Clerk-Maxwell was of a family, many members of which were talented, and not a few eccentric; to the latter class he himself belonged. He took an interest in all useful processes, and was successful in extending and improving the stony and mossy land which had become his by inheritance. The mother of James Clerk Maxwell belonged to an old family of the north of England, and was a woman of practical ability.

Glenlair was the name given to the new mansion and improved estate. Here the boy had every opportunity of becoming intimate with the ways of nature. He traversed the

* This Lecture was delivered on March 14, 1902.—Editors.

country with the help of a leaping pole, he navigated the duck pond in a washtub, he rode a pony behind his father's phaeton, he explored the potholes and grooves in the stony bed of the mountain stream which flowed past the house. He studied the ways of cats and dogs; he watched the transformation of the tadpole into the frog, and he imitated the manner in which a frog jumps. But he attracted attention not so much as an incipient naturalist as a physicist. He had great work with doors, locks and keys, and his constant request was "Show me hoo it doos." He investigated the course of the water from the duckpond to the river, and the courses of the bell-wires from the pulls to the bells in the kitchen, "action at a distance" being no explanation to him. When a very small boy he found out how to reflect the sun into the room by means of a tin-plate. He early acquired manual skill by making baskets, knitting elaborate designs and taking part in such other operations as went on around him, whether in the parlor, the kitchen, or on the farm.

Being an only child, young Maxwell made playmates of the children of the workmen on the farm, which had one bad effect; the Scottish dialect became such a native tongue that in after life he could not get rid of the brogue.

His early instruction in the elements of education was received from his mother. She taught him to read, stored his mind with Scripture knowledge, and trained him to look up through Nature to Nature's God. But she died from cancer at the early age of 48, and James was left when nine years old to the sole charge of his father. Education at home under a tutor was first tried, but the result was such that preparations were made to send him to the Edinburgh Academy, one of the best secondary schools of the Scottish metropolis. He entered the Academy in the middle of a term, and his reception by the other boys was not auspicious. His manners were not only rustic but eccentric; he had a hesitation in his speech, and he was clad more for comfort than for fashion. They were all dressed in round jacket and collar, the regulation dress for boys in the public schools of England; he came in a gray

tweed tunic and frill; and his shoes were made after a peculiar design of his father's with square toes and brass buckles. So at the first recess, when they were all outside, they came about him like bees, and demanded who made his shoes, to which he replied:

Din ye ken, 'twas a man,
And he lived in a house
In whilk was a mouse.

They tore his tunic and frill, and gave him the uncomplimentary nickname of "Dafty." *Daft* is a Scottish word meaning deficient in sense, or silly. Such was the first reception at public school of the boy who became the greatest mathematical electrician of the nineteenth century, whose electrical work in historical importance has been judged second only to that of Faraday. Had the annoyance to which young Maxwell was exposed been confined to the first few days at school, it might be set down to that disposition to haze newcomers which appears to be part of a boy's nature whether in the Old World or the New; but it was too generally persisted in, with the result that young Maxwell never quite amalgamated with the rest of the boys. There were, however, some exceptional lads who could appreciate his true worth, conspicuous among whom were Peter Guthrie Tait, afterwards Professor Tait, and Lewis Campbell, who became his biographer.

The curriculum at the Academy was largely devoted to Latin and Greek; and young Maxwell made a bad start in these subjects. A want of readiness, corresponding, I suppose, to the hesitation in his speech, kept him down, even in arithmetic. But about the middle of his school career he surprised his companions by suddenly becoming one of the most brilliant among them, gaining high, and sometimes the highest prizes for scholarship, mathematics and English verse composition. At his home in Edinburgh, his aunt's house, he had a room all to himself; it was not a study merely, but a laboratory. There before he had entered on the study of Euclid's *Elements* at the Academy he made out of pasteboard models of the five regular solids.

But while still a school boy he achieved a mathematical feat which was much more brilliant. His father was a member of the Scottish Society of Arts and of the Royal Society of Edinburgh, and it was his custom to take his son with him to the meetings, and indeed on visits to all places of scientific and industrial interest. A prominent member of the Society of Arts, Mr. D. R. Hay, a decorative painter, and author of a book *First Principles of Symmetrical Beauty* read a paper before that Society on how to draw a perfect oval. His method was by means of a string passing round three pegs. Young Maxwell had by this time entered on the study of the Conic Sections, and he took up the problem in his laboratory. He modified the manner of tracing an ellipse by doubling the cord from the tracing-point to one of the foci; the curve then described is the oval of Descartes. He also found out how to do it when twice the distance from one focus plus three times the distance from the other focus is to be constant. Maxwell's father wrote out an account of his son's method, and gave it to J. D. Forbes, then professor of natural philosophy at the University of Edinburgh, and Secretary of the Royal Society of Edinburgh. Both Forbes, and Kelland, the professor of mathematics, approved the paper as containing something new to science; it was read by Forbes at the next meeting of the Society, and is printed in the second volume of the *Proceedings* under the title " On the description of oval curves, and those having a plurality of foci; By Mr. Clerk-Maxwell, Jr., with remarks by Professor Forbes." The author was then 15 years of age. Next year (1847) he finished the curriculum at the Academy, first in mathematics and in English, and very nearly first in Latin.

He now became a student of the University of Edinburgh. At that time the curriculum in Arts embraced seven subjects: Latin, Greek, Mathematics, Physics, Logic and Metaphysics, Moral Philosophy, English Literature. Maxwell made a selection skipping Latin, Greek, and English Literature. Kelland was the professor of mathematics, Forbes of physics, Sir William Hamilton of logic and metaphysics; under these he studied for two years. To Kelland and Forbes he was already known,

and the latter gave him the special privilege of working with the apparatus used in the lectures on physics. There was then no well-appointed physical laboratory; any research made was conducted in the lecture room or the room for storing the lecture apparatus. But strange as it may seem, Maxwell appears to have done most work for the class of logic. Sir William Hamilton (that is, the Scottish baronet) was noted for his attack on mathematics as an educational discipline, but he was learned in scholastic logic and philosophy, and he had the power of inspiring his students. It was his custom to print on a board the names of the best students for the year in the order of merit; I recollect seeing on one board the name of James Clerk Maxwell, I think about sixth in the list. About this time George Boole published his *Mathematical Analysis of Logic* which found in Maxwell an appreciative reader. In his third year at the University, besides continuing his experiments in the physical department, he took Moral Philosophy under Professor Wilson, who wrote much under the name of Christopher North but whose lectures on moral science were characterized by Maxwell as vague harangues; also Chemistry in the department of Medicine, and there, as in Physics, he was privileged to make experiments. The academic session at Edinburgh is short—only six months; the long vacations he spent at Glenlair, where he fitted up a small laboratory in the garret of the former dwelling house. There he studied and experimented on the phenomena of light, electricity and elasticity. As the outcome of these researches he contributed two papers to the Royal Society of Edinburgh, which were printed in the *Transactions;* one on "The Theory of Rolling Curves," the other on "The Equilibrium of Elastic Solids." During his study at Edinburgh University, Maxwell made great use of the high-class works on mathematics and physics which were to be found in the University Library, acting unconsciously on the advice of his compatriot and subsequent neighbor—Thomas Carlyle.

In sending his son to Edinburgh University it was John Maxwell's intention to educate him for the legal profession—

to become an advocate like himself. But the youth's success as an investigator in mathematics and physics suggested to such friends as Forbes, Kelland, Thomson and Blackburn, a scientific career, and it was Maxwell's own conviction that he was better fitted to grapple with the laws of nature than with the laws of the land. His former school fellow Tait, after studying mathematics and physics for one brief session at the University of Edinburgh, had taken up the regular course of study at the University of Cambridge; and he wished to follow. His father was at length persuaded, with the result that Maxwell became a member of St. Peter's College, Cambridge, at the age of 19. Tait was a member of the same college, now entering on the third and last year of his undergraduate course. Thomson was now a fellow of that college.

The change to Cambridge involved a great discontinuity; and Maxwell by nature loved continuity in all his life and surroundings. The investigator of rolling curves and the compression of solids was now obliged to turn his attention again to the *Elements* of Euclid, and to finding out by the aid of lexicon and grammar the meaning of a Greek play. But, worse still, he found that his fellow students in Peterhouse had no sympathy with physical manipulations. He had brought with him from his laboratory a pair of polarizing prisms, the gift of the inventor Nicol, pieces of unannealed glass, magnets, jampots, guttapercha, wax, etc.; why he should fool with these things was beyond the comprehension of the young gentlemen who lodged and studied in the same college. At the end of his first year Maxwell migrated to Trinity College, the largest foundation of the University, then governed by Whewell who had a broad interest in all the sciences. Physical experimenting was not then so fashionable at Cambridge as it is now; Newton, indeed, made his experiments on light in Trinity College, but very little had been done since his days. In the college of Newton, Maxwell found not only congenial spirits, but soon came to be looked up to as a leader by a set of admiring followers. During his undergraduate years Maxwell found time to contribute various papers to the *Cambridge and*

*Dublin Mathematical Journal;* he was also elected into the Apostles' Club; so-called from the number of the members; their object was the discussion of philosophical questions.

After passing the Little-go, that is the examination in the preliminary studies, he went into training for the mathematical tripos, placing himself in the care of the great trainer of the day, William Hopkins. Notwithstanding that he turned aside often to his favorite pursuits, he succeeded by sheer strength of intellect in gaining the place of second wrangler; and in the more severe competition for the Smith's prizes he was bracketed equal with the senior wrangler. His rival was Routh, who subsequently became the leading tutor for the mathematical tripos, and in the mathematical world is known as the author of a treatise on *Rigid Dynamics.* Released from a course of prescribed study and the tyranny of a mathematical trainer, Maxwell rebounded at once to his much-loved researches. The spirit in which he now entered upon his independent career as an investigator may be gathered from an aphorism which he wrote for his own conduct: "He that would enjoy life and act with freedom must have the work of the day continually before his eyes. Not yesterday's work, lest he fall into despair, not to-morrow's, lest he become a visionary—not that which ends with the day, which is a worldly work, nor yet that only which remains to eternity, for by it he cannot shape his action. Happy is the man who can recognize in the work of to-day a connected portion of the work of life, and an embodiment of the work of eternity. The foundations of his confidence are unchangeable, for he has been made a partaker of Infinity. He strenuously works out his daily enterprises, because the present is given him for a possession."

His activity took two principal directions—optical and electrical. For the former line of investigation he inquired on all sides for color-blind persons, devised an instrument for examining the living retina, which he was specially successful in applying to the dog; read Berkeley's *Theory of Vision* and that part of Mill's *Logic* which treats of the relation of sensation to knowledge; perfected his color top and made an

extended series of observations with it. Maxwell's color top consists of a heavy disk with perpendicular spindle. Sectors of different colored papers can be placed on the disk, and made to overlap more or less; a smaller colored disk can be attached so as to cover the central part only. When the top is made to spin, the reflected colors which succeed one another in position are mixed in the eye, and the mind perceives a uniform color. The angular lengths are adjusted till, if possible, a match is made with the color in the centre; then the color equation is read off.

As regards the electrical line of investigation he had already conceived the idea of making the old mathematical theory of electrical attraction and repulsion, as elaborated by Coulomb and Poisson, harmonize with the method by which Faraday was obtaining splendid results, namely, the consideration of the lines of force in the medium. With this end in view he studied the German and French writers; and in the winter of 1855–56 he published a paper on Faraday's lines of force.

At the age of 24 he gained, after competitive examination, a fellowship from his college. Soon after, the chair of physics (natural philosophy it is there called) in Marischel College, one of the teaching colleges of the University of Aberdeen, Scotland, fell vacant; and Maxwell was advised by his old friend Forbes to become a candidate for the appointment. The suggestion agreed with his own aims as to a career, and he found that his father also approved of it. He sent in his application; and was appointed but not before his father had died. So, in the spring of 1856 he became both the master of Glenlair and the professor of physics in Marischel College, Aberdeen University.

He entered on his teaching work at Aberdeen with great enthusiasm. A professor in the Scottish Universities is free to teach his subject according to the most approved method, and is not bound to bend all energies towards fitting his students for an examination conducted by independent examiners; this feature of his duties Maxwell valued highly. At Cambridge he had taken a share in lectures to workingmen, and at Aberdeen

he continued the practice. While he was very skillful as an experimenter, he was not so successful as an expositor. He had received no training as a teacher; following the example of his father he was accustomed to present things after a curiously grotesque fashion; his vision was short-sighted; his speech was not free from hesitation; his imagination outran his vocabulary; and he could not easily put himself at the viewpoint of the average student attending his lectures.

During the next year he was married to Katherine Dewar, daughter of the principal of the college and a Presbyterian divine, sister I believe of James Dewar who in recent years has become famous for his investigation of the properties of bodies at temperatures bordering on the absolute zero.

St. John's College, Cambridge, had founded an Adams prize in honor of the discoverer of Neptune, to be awarded to the writer of the best essay on a prescribed subject, and to be open to all graduates of the University. In 1857 the examiners chose for the subject "The motion of Saturn's rings." Maxwell made an elaborate investigation, and his essay carried off the prize.

Galileo in 1610 by means of his small telescope discovered a pair of satellites attached to the planet, one on either side. Huyghens in 1659 resolved the pair of satellites into a continuous ring. Cassini in 1679 resolved the continuous ring into an outer and inner ring. Herschel in 1789 determined the period of rotation of the outer ring. In 1850 a dusky ring within the inner bright ring was discovered by Bond at Cambridge, Mass. Maxwell opens his essay as follows: "When we contemplate the rings of Saturn from a purely scientific point of view, they become the most remarkable bodies in the heavens, except, perhaps those still less useful bodies the spiral nebulæ. When we have actually seen that great arch swung over the equator of the planet without any visible connection, we cannot bring our minds to rest. We cannot simply admit that such is the case, and describe it as one of the observed facts in nature not admitting or requiring explanation. We must either explain its motion on the principles of mechanics

or admit that, in the Saturnian realms, there can be motion regulated by laws which we are unable to explain." Maxwell then showed that the rings, if either solid or liquid, would break into pieces, and concluded as follows: "The final result of the mechanical theory is, that the only system of rings which can exist is one composed of an indefinite number of unconnected particles, revolving round the planet with different velocities according to their respective distances. These particles may be arranged in series of narrow rings, or they may move through each other irregularly. In the former case the destruction of the system will be very slow; in the second case it will be more rapid, but there may be a tendency towards an arrangement in narrow rings, which may retard the process." It follows from Maxwell's theory that the inner ring must have a greater angular velocity than the outer ring; and that this is the fact was later shown by Keeler at the Allegheny Observatory.

Aberdeen was the meeting place of the British Association in 1859. William Rowan Hamilton was there, full of his new method of quaternions; also Tait, now professor of mathematics at Belfast, and a disciple of Hamilton's. Maxwell was introduced to Hamilton by Tait. He had doubtless already studied the new method, from which he assimilated many ideas which figure largely in his *Treatise on Electricity and Magnetism.* At Aberdeen there are two colleges, Marischel College and King's College, each of which had then a Faculty of Arts. An agitation for a change had been in progress for some years; in 1860 it ended in a fusion of the two faculties of arts. The Kings College professor of physics was David Thomson, of whom you doubtless have never heard, yet Thomson was retained, and the gifted Clerk Maxwell was left out. However the Crown gave him compensation in the form of a pension. Just then Forbes resigned the chair of physics at Edinburgh; the two friends Maxwell and Tait were rival candidates, and Tait was successful. The contest did not change their friendship. Maxwell was immediately appointed to the corresponding chair in King's College, London.

In London his duties were not so congenial as they had been in Aberdeen. The session was much longer, and he was not so free to adopt his own methods, for the college was affiliated to the University of London, which alone had the power of granting degrees. After five years in this office he retired to his own estate. While in London he carried out three important investigations. He had already investigated the mixing of colors reflected from colored papers; he now took up the mixing of pure colors of the spectrum. For this purpose he made a wooden box 8 feet long, painted it black both inside and outside, fitted it with the necessary slits, prisms, and lenses; and, in order to get the necessary sunlight, placed it in the window of the garret of his house. Here he observed the effect of mixing the spectral tints, and his neighbors thought him mad to spend so many hours staring into a coffin.

His investigation of the stability of Saturn's rings introduced to his attention the flight of a countless horde of small solid bodies; from this to the kinetic theory of gases the transition is natural.

The third task was the construction for the British Association of a material ohm, defined as the resistance of a circuit when an electromotive force of one volt sends a current of one weber through it. Maxwell, more than any other man, was the founder of the C.G.S. system of units, which became the basis of that practical system of electrical units which is now legalized in all civilized countries. "Weber" was originally the name for a unit of current. In the last verse of his "Valentine from a male telegraphist to a female telegraphist," Maxwell introduces the newly defined units:

> Through many an ohm the weber flew,
> And clicked the answer back to me,—
> I am thy farad, staunch and true
> Charged to a volt with love for thee.

It was eminently appropriate that in 1900 the International Electrical Congress should give Maxwell's own name to the unit of magnetic flux.

For five years (1865–1870) he lived a retired life at Glenlair, broken by visits to London, Cambridge, Edinburgh, and the Continent. But it was then that he found leisure to complete the great work of his life the *Treatise on Electricity and Magnetism*, published in two volumes in 1873. The aim of the work is to give a connected and thorough mathematical theory of all the phenomena of electricity and magnetism. He started from the facts observed in Faraday's experiments, and in their light he read the old theory of electric action. This work has served as the starting point of many of the advances made in recent years. Maxwell is the scientific ancestor of Hertz, Hertz of Marconi and all other workers at wireless telegraphy. In the introductory chapter Maxwell remarks that the Earth (which was made the basis of the metric system) is not sufficiently constant either in form or in period of rotation; and advises physicists who may judge their papers worthy of a greater endurance to base their units upon the wave-length and period of some specified molecule. Humor or not, Michelson in this country has actually compared the meter of the archives with the wave-length of a certain ray of light.

Professor Tait gave Maxwell much assistance in the preparation of his great Treatise. He urged him to introduce the Quaternion method; but Maxwell found serious practical difficulties. For one thing Hamilton makes use of the Greek alphabet, and Maxwell found that all the Greek letters had already been appropriated to denote physical quantities. But Maxwell was an intuitionalist, and he never trusted to analysis beyond what he could picture clearly. So he adopted the rather curious middle course. "I am convinced that the introduction of the ideas, as distinguished from the operations and methods of Quaternions, will be of great use to us in all parts of our subject." In this departure we have the origin of the school of vector-analysts as opposed to the pure quaternionists.

In 1870 the Duke of Devonshire, who was Chancellor of the University of Cambridge, signified his desire to build and equip a physical laboratory. The Senate accepted the gift,

and founded in connection a chair of experimental physics. Sir William Thomson was invited to become a candidate, but declined; Maxwell was invited, and after some hesitation acceded. He was elected without opposition. For some time after his appointment Maxwell's principal work was that of designing and superintending the erection of the Cavendish Laboratory, so-called after the family name of the donor. It was opened in 1874. In the following vacation I visited it, but Maxwell as was his wont, had gone to his country home. His assistant mentioned that the equipment was far from complete, and that they were afraid that the Duke of Devonshire might die before his promise of a complete equipment had been availed of. It was not till 1877 that the equipment was completed.

Soon after (1873) he became Cavendish professor he delivered the famous "Discourse on Molecules" in an evening lecture before the British Association, then assembled in Bradford. Maxwell viewed the doctrine of evolution, or at any rate the extreme consequences deducible from that doctrine, with marked disfavor. This dislike originated in part from his bias as a Christian and a theist, but it rested also on philosophical convictions which he set forth in this address. The conclusion is as follows: "In the heavens we discover by their light, and by their light alone, stars so distant from each other that no material thing can ever have passed from one to another; and yet this light, which is to us the sole evidence of the existence of these distant worlds, tells us also that each of them is built up of molecules of the same kinds as those which we find on earth. A molecule of hydrogen, for example, whether in Sirius or in Arcturus, executes its vibrations in precisely the same time. Each molecule therefore throughout the universe bears impressed upon it the stamp of a metric system as distinctly as does the meter of the Archives at Paris, or the double royal cubit of the temple of Karnac. No theory of evolution can be formed to account for the similarity of molecules, for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction.

None of the processes of Nature, since the time when Nature began, have produced the slightest difference in the properties of any molecule. We are therefore unable to ascribe either the existence of the molecules or the identity of their properties to any of the causes which we call natural. On the other hand, the exact equality of each molecule to all others of the same kind gives it, as Sir John Herschel has well said, the essential character of a manufactured article, and precludes the idea of its being eternal and self existent."

The next year, 1874, a counterblast was delivered by Prof. Tyndall in his address at Belfast, as president of the Association. In it occurs the following passage: "Believing as I do, in the continuity of Nature, I cannot stop abruptly where our microscopes cease to be of use. Here the vision of the mind authoritatively supplements the vision of the eye. By an intellectual necessity I cross the boundary of the experimental evidence, and discern in that Matter which we, in our ignorance of its latent powers, and notwithstanding our professed reverence for its Creator, have hitherto covered with opprobrium, the promise and potency of all terrestrial life." Maxwell was present, and he sent to Blackwood's Magazine verses entitled "Notes of the president's address" in which the different points of the address are hit off very nicely.

After accepting the Cavendish professorship he unfortunately took on hand the work of editing the unpublished electrical researches, made 100 years before, by the Hon. Henry Cavendish, a member of the Devonshire family. The task cost him much time and labor which could have been better spent on his own unfinished projects, one of which was an Experimental Treatise on Electricity and Magnetism.

Mrs. Maxwell was now an invalid, and depended much on his care. In the spring of 1879 he himself began to be troubled with dyspeptic symptoms, especially with a painful choking sensation after eating meat; in the fall he sent for an Edinburgh physician to come to Glenlair, and was then informed that he had only a month to live. To get the best medical attention, he and his wife set out for Cambridge; the worst

features of his suffering were alleviated, and his intellect remained unclouded to the last. He died on the 5th of November, 1879, having nearly completed the 48th year of his age. It is supposed that he inherited the same disease which had caused the untimely death of his mother. He was buried in Parton churchyard among the Maxwells of Middlebie. He left no descendants. Mrs. Maxwell lingered a few years longer, and she bequeathed the residue of her estate to founding a scholarship for experimental work in the Cavendish Laboratory. In the laboratory there is a bust of its first professor, and what is of greater interest, the collection of the models and apparatus which he made with his own hands. Maxwell's portrait hangs in the dining hall of Trinity College, alongside that of Cayley. He was the founder and benefactor of a Presbyterian Church near his home; there he used to officiate as an elder, and in that church there is now a window in his memory.

Since the time of his death his fame has grown immensely, especially in consequence of the wonderful applications made of his electro-magnetic theory. That theory led to the conclusion that the velocity of propagation of electrical disturbances is the same as the velocity of light, that light itself is an electro-magnetic phenomenon, and that the ratio of the units of the electro-magnetic and electro-static units is the same as the velocity of light in a vacuum. In 1873 he predicted that in the discharge of a Leyden jar electric waves would be produced in the ether, and in 1879 such waves were detected experimentally by Hertz. As a consequence wireless telegraphy is now possible across the Atlantic Ocean.

# WILLIAM JOHN MACQUORN RANKINE *

(1820–1872)

WILLIAM JOHN MACQUORN RANKINE was born in Edinburgh, Scotland, on the 5th of July, 1820. He was by descent a Scot of Scots. His father, David Rankine, descended from the Rankines of Carrick, could trace his descent back to Robert the Bruce. Carrick is a hill district of Ayrshire in the southwest of Scotland, famous for its breed of dairy cattle. Before his accession to the Crown of Scotland, Robert the Bruce was Earl of Carrick. In youth Rankine's father was a lieutenant in the regular army, but later in life he became a railroad engineer and eventually Secretary of the Caledonian Railway Company. His mother was Barbara Graham, daughter of a Glasgow banker, and second cousin of Thomas Graham who is celebrated for his investigation of the diffusion of gases and liquids.

Rankine spent his first years in Ayrshire among the Carrick Hills, which he afterwards celebrated in verse, for Rankine, like Maxwell, was an amateur poet:

Come busk ye braw, my bonnie bride,
And hap ye in my guid gray plaid,
And ower the Brig o' Doon we'll ride
Awa' to Carrick Hills, love.

For there's flowery braes in Carrick land,
There's wimplin' burns in Carrick land,
And beauty beams on ilka hand
Amang the Carrick Hills, love.

* This Lecture was delivered on March 18, 1902.—EDITORS.

There dwalt my auld forefathers lang,
Their hearts were leal, their arms were strang,
To thee my heart and arm belang
Amang the Carrick Hills, love.

I'll bear thee to our auld gray tower,
And there we'll busk a blythesome bower,
Where thou shalt bloom, the fairest flower,
Amang the Carrick Hills, love.

In spring we'll watch the lammies play,
In summer ted the new-mown hay,
In harvest we'll sport the lee-lang day
Amang the Carrick Hills, love.

When winter comes wi' frost and snaw,
We'll beet the bleeze, and light the ha',
While dance and song drive care awa'
Amang the Carrick Hills, love.

In these verses we have a description by Rankine of the scenes and pastimes in which he spent his earliest years. Carrick borders on Galloway, and there, ten years later, Clerk-Maxwell grew up in a similar environment. After some preliminary education at home he was sent when eight years of age, to the public school; first to the Academy of the neighboring town of Ayr, afterward to the High School of the City of Glasgow. But his health broke down, and he was restricted for some years to private instruction at his home now in Edinburgh. To his father he was indebted for superior instruction in arithmetic, elementary mathematics, mechanics and physics. When 14 years of age he received from his mother's brother a present, which had a powerful effect on his subsequent career —a copy of Newton's *Principia*. To his private study of that book and of other books of the like order, he was indebted for his skill in the higher mathematics. While his education proceeded at home, he received instruction in the composition and playing of music, which enabled him in after years to compose the tunes for his own songs.

At the age of 16 he entered the University of Edinburgh. Instead of taking a regular course, he selected chemistry, physics, zoology and botany. Forbes was then the professor of physics; Rankine attended his class twice; the first year he received the gold medal for an essay on "The Undulatory Theory of Light," and the subsequent year an extra prize for one on "Methods of Physical Manipulation." It appears that he did not enter any class of pure mathematics at the University, having already advanced beyond the parts then taught. At this time he was attracted, like many other mathematicians at the beginning of their independent career, by the theory of numbers. In his leisure time he studied extensively the works of Aristotle, Locke, Hume, Stewart, and other philosophers.

Before finishing his studies at the University of Edinburgh, he had gained some practical experience by assisting his father in his work as a railroad engineer. There was then no professor of engineering at the University of Edinburgh; some 20 years later Fleeming Jenkin was appointed, and given that whole province which is now divided at this University into four great departments. Hence, at the age of 18, Rankine was made a pupil of Sir John Macneill, civil engineer, and as a pupil he was employed for four years on various surveys and schemes for river improvements, waterworks, and harbors in Ireland. It was then that he became personally acquainted with the "gorgeous city of Mullingar," which he has described minutely and gracefully in an ode to its praise. He was likewise employed on the construction of the Dublin and Drogheda railway and it was while so engaged that he contrived the method of setting out curves which is known as Rankine's method.

Having finished his term of pupilage, he returned to his father's home in Edinburgh, and commenced the practice of his profession. One of the first projects entrusted to his care was rather singular. In 1842 Queen Victoria visited Scotland for the first time, and resided for several days in the home of her Stuart ancestors—Holyrood Palace in Edinburgh. Royal visits to Scotland were not so frequent then as they afterwards became. One manifestation of rejoicing took the form of a

large bonfire on the top of Arthur's Seat, a precipitous rock which rises 700 feet above the level of the park surrounding the palace. To Rankine was entrusted the engineering of this bonfire. Applying his knowledge of chemistry he constructed the pile of fuel with radiating air passages underneath.

It was now, when he was 22 years of age, that he published his first scientific pamphlet—"An experimental inquiry into the advantage of cylindrical wheels on railways." The course of experiments was suggested by his father, and was carried out by father and son working together. It was followed by a series of papers on subjects suggested by his father's railroad experience; of which one was on the "Fracture of axles." He showed that such fractures arose from gradual deterioration or fatigue, involving the gradual extension inwards of a crack originating at a square-cut shoulder. In this paper the importance of continuity of form and fiber was first shown, and the hypothesis of spontaneous crystallization was disproved. His father was connected with the Caledonian Railway Company, and by that Company young Rankine was professionally employed on various schemes. The work in Ireland had impressed on him the great importance of an abundant supply of pure water to the health of a city. He brought forward a scheme for supplying the city of Edinburgh with water from a lake in the hilly region to the south; a scheme which was thorough and would have solved the problem once and for all. It was defeated by the existing Water Company, with the result that to this day the water supply of the city of Edinburgh is defective.

While engaged in engineering work in Ireland, he had thought much on the mechanical nature of heat, a doctrine which was then engaging the attention of the scientific world. In reading the *Principia* of Newton, Rankine must have observed how the action of heat was a difficulty in the theory of Dynamics. In France, Carnot had in 1820 given a theory of the heat-engine which assumed that heat was a material substance. Mayer had advanced the theory that heat is a mode of motion. Rankine to explain the pressure and expansion of gaseous

substances due to heat, conceived the *hypothesis of molecular vortices.* He worked out his theory, but owing to the want of experimental data, did not publish immediately. In 1845 Joule brought to a successful result a series of experimental investigations designed to measure the exact mechanical equivalent of a given amount of heat. In 1849 William Thomson, professor of physics at Glasgow, gave an account to the Royal Society of Edinburgh of Carnot's theory, and the problem then was, "How must the theory of the heat-engine be modified, supposing that heat is not a substance, but a mode of motion?" Rankine reduced his results to order, and contributed them to the Royal Society of Edinburgh in two papers entitled "On the mechanical action of heat, especially in gases and vapors" and "The centrifugal theory of elasticity as applied to gases and vapors." He was elected a fellow, and read his papers early in 1850. That same year the British Association met in Edinburgh. Rankine was Secretary of Section A, and he had ready an elaborate paper "On the laws of the elasticity of solid bodies," in which the same hypothesis of molecular vortices is the guiding idea.

Rankine was not content to suppose the heat of a body to be the energy of the molecules due to some kind of motion. He supposed, like the other pioneers in thermodynamics, that the invisibly small parts of bodies apparently at rest are in a state of motion, the velocity of which, whether linear or angular, is very high. But he went further; he imagined the motion to be like that of very small vortices each whirling about its own axis; from which it would follow that the elasticity of a gas is due to the centrifugal force of this motion; an increase of angular velocity would mean an increase of centrifugal force. His own statement of the hypothesis is as follows: "The hypothesis of molecular vortices may be defined to be that which assumes that each atom of matter consists of a nucleus or central point enveloped by an elastic atmosphere, which is retained in its position by attractive forces, and that the elasticity due to heat arises from the centrifugal force of those atmospheres, revolving or oscillating about their under or central points." Rankine's

molecular vortex is the attracting point of Boscovich surrounded by an elastic atmosphere.

Maxwell wrote in *Nature* in 1878: "Of the three founders of theoretical thermodynamics (Rankine, Thomson, Clausius) Rankine availed himself to the greatest extent of the scientific use of the imagination. His imagination, however, though amply luxuriant, was strictly scientific. Whatever he imagined about the molecular vortices with their nuclei and atmospheres was so clearly imaged in his mind's eye, that he, as a practical engineer, could see how it would work. However intricate, therefore, the machinery might be which he imagined to exist in the minute parts of bodies, there was no danger of his going on to explain natural phenomena by any mode of action of this machinery which was not consistent with the general laws of mechanism. Hence, though the construction and distribution of his vortices may seem to us as complicated and arbitrary as the Cartesian system, his final deductions are simple, necessary, and consistent with facts. Certain phenomena were to be explained. Rankine set himself to imagine the mechanism by which they might be produced. Being an accomplished engineer, he succeeded in specifying a particular arrangement of mechanism competent to do the work, and also in predicting other properties of the mechanism which were afterwards found to be consistent with observed facts."

In his paper on the "Mechanical Action of Heat," Rankine applied the dynamical theory of heat and his hypothesis of molecular vortices, to discuss new relations among the physical properties of bodies, and especially to a relation between the true specific heat of air, the mechanical equivalent of heat, and certain other known constants. He found, using the value for the mechanical equivalent which had just been published by Joule, that the true specific heat of air relative to that of water has the value 0.2378. The best value for that quantity which had been obtained by direct experiment was that of De la Roche and Bérard, 0.2669. Rankine concluded, not that his theory was wrong, but that Joule's result was too small. On further examination of Joule's investigation, just printed in

the *Philosophical Transactions*, he concluded that De la Roche and Bérard's value was too large; and predicted that the true specific heat of air would be found to be 0.2378. Three years later Regnault obtained by direct experiment the value 0.2377.

Soon after this he moved to Glasgow, and founded there the firm of Rankine and Thomson, civil engineers. They took up a scheme for supplying the City of Glasgow with water from Loch Katrine. They were not the originators of the scheme, but they were successful in carrying it out. The City of Glasgow solved effectively the problem of an abundant supply of pure water; and in so doing commenced a career which has made it the model municipality of the British Islands. As a resident of Glasgow he became an active member of the Philosophical Society of Glasgow; and to that Society he contributed in 1853 one of his most important memoirs "The general law of the transformation of energy." Two years later he contributed "Outlines of the science of energetics," on the abstract theory of physical phenomena in general, which has now become the logical foundation for any treatise on physics. In it he introduces and defines exactly a number of terms which were then strange or altogether new, but are now familiar concepts in physical science, such as "actual energy" and "potential energy."

To the doctrines of the Conservation and Transformation of Energy, Prof. William Thomson added the doctrine of the dissipation of energy. This doctrine asserts that there exists in nature a tendency to the dissipation or uniform diffusion of mechanical energy originally collected in stored up form; in consequence of which the solar system (and the whole visible universe) tends towards a state of uniformly diffused heat; in which state according to the laws of thermodynamics no further transformation of energy is possible; in other words, nature tends towards a state of universal death. Rankine speculated as to how this dire result may be provided against in nature, and contributed to the meeting of the British Association, held at Belfast in 1852 a paper "On the reconcentration of the mechanical energy of the universe." "My object,"

he said, " is to point out how it is conceivable that, at some indefinitely distant period, an opposite condition of the world may take place, in which the energy which is now being diffused may be reconcentrated into foci, and stores of chemical power again produced from the inert compounds which are now being continually formed. There must exist between the atmospheres of the heavenly bodies a material medium capable of transmitting light and heat; and it may be regarded as almost certain that this interstellar medium is perfectly transparent and diathermanous; that is to say, that it is incapable of converting heat or light from the radiant into the fixed or conductible form. If this be the case, the interstellar medium must be incapable of acquiring any temperature whatever, and all heat which arrives in the conductible form at the limits of the atmosphere of a star or planet, will there be totally converted, partly into ordinary motion by the expansion of the atmosphere, and partly into the radiant form. The ordinary motion will again be converted into heat, so that *radiant heat* is the ultimate form to which all physical energy tends; and in this form it is, in the present condition of the world, diffusing itself from the heavenly bodies through the interstellar medium. Let it now be supposed, that, in all directions round the visible world, the interstallar medium has bounds beyond which there is empty space. If this conjecture be true, then on reaching those bounds, the radiant heat of the world will be totally reflected, and will ultimately be reconcentrated into foci. At each of these foci the intensity of heat may be expected to be such, that should a star (being at that period an extinct mass of inert compounds) in the course of its motions arrive at that point of space, it will be vaporized and resolved into its elements; a store of chemical power being thus reproduced at the expense of a corresponding amount of radiant heat. Thus it appears, that although, from what we can see of the known world, its condition seems to tend continually towards the equable diffusion in the form of radiant heat, of all physical energy, the extinction of the stars, and the cessation of all phenomena; yet the world, as now created, may possibly be provided within

itself with the means of reconcentrating its physical energies, and renewing its activity and life. For aught we know, these opposite processes may go on together, and some of the luminous objects which we see in distant regions of space may be, not stars, but foci in the interstellar ether."

In 1853 Rankine was elected a Fellow of the Royal Society of London; and in the following year he sent to that Society one of his important memoirs "The geometric representation of the expansive action of heat."

Glasgow University was in advance of the Edinburgh University in having a chair of civil engineering and mechanics. At the beginning of 1855 the incumbent of the chair was incapacitated by ill health, and Rankine acted as substitute for the remainder of the session. That same year at the age of 35 he was appointed to the chair.

Professor Rankine has been described by an intimate friend, Professor Tait: "His appearance was striking and prepossessing in the extreme, and his courtesy resembled almost that of a gentleman of the old school. His musical tastes had been highly cultivated, and it was always exceedingly pleasant to see him take his seat at the piano to accompany himself as he sang some humorous or grotesquely plaintive song—words and music alike being generally of his own composition. His conversation was always interesting, and embraced with equal seeming ease all topics, however various. He had the still rarer qualification of being a good listener also. The evident interest which he took in all that was said to him had a most reassuring effect on the speaker, and he could turn without apparent mental effort from the prattle of young children to the most formidable statement of new results in mathematical or physical science, when his note-book was at once produced, and in a few lines he jotted down the essence of the statement, to be pondered over at leisure, provided it did not at once appear to him how it was to be modified. The questions which he asked on such occasions were always almost startlingly to the point, and showed a rapidity of thought not often met with in minds of such caliber as his, where the mental inertia which

enables them to overcome obstacles, often prevents their being quickly set in motion. His kindness, shown in the readiness with which he undertook to read proof sheets for a friend, or even to contribute a portion of a chapter (when the subject was one to which he had paid special attention) was, for a man so constantly at work, absolutely astonishing."

It is customary in the Scottish Universities for a new professor to deliver an inaugural lecture on some subject of general interest connected with his chair; and at that time the discourse was in the Latin language. Professor Rankine chose for his subject "De concordia inter scientiarum machinalium contemplationem et usum"; or the concord in the mechanical sciences between theory and practice; it is printed as a preliminary dissertation in his *Manual of Applied Mechanics.* In it he traces from ancient down to medieval times the course of the fallacy that there is a *double system of natural laws*, one theoretical, geometrical, rational, discoverable by contemplation, applicable to celestial ethereal indestructible bodies, and a fit object for the noble and liberal arts; the other system practical, mechanical, empirical, discoverable by experience, applicable to terrestrial gross destructible bodies, and fit only for what were once called the vulgar and sordid arts. And he showed that this fallacy, although no longer formally maintained, still exerted an influence. In reference to this, Professor Greenhill has observed "Although the double system of natural laws mentioned by Rankine is now exploded, we still have a double system of instruction in mechanical textbooks, one theoretical, general, rational; the other practical, empirical, discoverable by experience. It should be the object of modern science to break down the barriers between these two systems, and to treat the subject of mechanics from one point of view."

Appointed to the chair of engineering, Rankine was soon the recipient of many honors. He was made president of the section of engineering, when the British Association met in Glasgow; and the following year, on the occasion of their meeting in Dublin, he received from the University of Dublin the honorary degree of LL.D. The following year he was chosen

the first president of the Institution of Engineers in Scotland, an organization of which he had been a principal promoter. Professor Rankine had by this time abundantly proved himself as a pathfinder in the undiscovered regions of science; he was now to prove himself as a roadmaker. His practice as an engineer had made him fully alive to the important difference between the crude results of theoretical reasoning from principles and the reduced formulas adapted to the data obtainable from observation or specification. No sooner was he settled in his chair, than he began the preparation of his celebrated series of engineering manuals. In 1857 appeared *Applied Mechanics;* in 1859 *Steam-engine;* in 1861 *Civil Engineering;* in 1869 *Machinery and Mill Work;* supplemented in 1866 by *Useful Rules and Tables.* These manuals have gone through many editions, and there is still a demand for them. Why this phenomenal success? Professor Tait answered, "Rankine was peculiarly happy in discriminating between those branches of engineering knowledge which grow from daily experience, and those which depend on unchangeable scientific principles. In his books he dealt almost exclusively with the latter, which may, and certainly will, be greatly extended, but so far as they have been established can never change. . . . Really original papers and monographs rapidly lose their interest and importance, except as historical landmarks, but Rankine's works will retain their value after this generation has passed away."

In 1859 the volunteer movement spread over Great Britain. In view of possible invasion of the country it was thought that the regular army and the militia ought to be supplemented by bodies of trained citizens; the motto was *for defence, not defiance.* The movement spread to the University of Glasgow, and Rankine, true to transmitted instincts, gave in his name. He was made captain, and rose to be senior major; but after serving for five years he was obliged to resign on account of the pressure of his professional duties and of the labor involved in the preparation of the manuals. In 1861 he was made president of the Philosophical Society of Glasgow, and from the

chair he delivered an address "On the use of mechanical hypothesis in science, especially in the theory of heat." The address shows a clear appreciation of the logical bearing of scientific hypothesis. He had been criticised for holding the hypothesis of molecular vortices. "In order to establish," he said, "that degree of probability which warrants the reception of a hypothesis into science, it is not sufficient that there should be a mere loose and general agreement between its results and those of experiment. Any ingenious and imaginative person can frame such hypotheses by the dozen. The agreement should be mathematically exact to that degree of precision which the uncertainty of experimental data renders possible, and should be tested in particular cases by numerical calculation. The highest degree of probability is attained when a hypothesis leads to the prediction of laws, phenomena and numerical results, which are afterwards verified by experiment, as when the wave-theory of light led to the prediction of the true velocity of light in refracting media, of the circular polarization of light by reflection, and of the previously unknown phenomena of conical and cylindrical refraction; and as when the hypothesis of atoms in chemistry led to the prediction of the exact proportions of the constituents of innumerable compounds. . . . I think I am justified in claiming for the hypothesis of molecular vortices, as a means of advancing the theory of the mechanical action of heat, the merit of having fulfilled the proper purposes of a mechanical hypothesis in physical science, which are to connect the laws of molecular phenomena by analogy with the laws of motion; and to suggest principles such as the second law of thermodynamics and the laws of the elasticity of perfect gases, whose conformity to fact may afterwards be tested by direct experiment. And I make that claim the more confidently that I conceive the hypothesis in question to be in a great measure the development and the reduction to a precise form of ideas concerning the molecular condition which constitutes heat, that have been entertained from a remote period by the leading minds in physical science. . . . I wish it, however, to be clearly understood, that although I

attach great value and importance to sound mechanical hypotheses as means of advancing physical science, I firmly hold that they can never attain the certainty of observed facts; and, accordingly, I have labored assiduously to show that the two laws of thermodynamics are demonstrable as facts, independent of any hypothesis; and in treating of the practical application of those laws, I have avoided all reference to hypothesis whatever."

The pressure of a gas is now explained by the impacts and collisions of the molecules. But a sound hypothesis, although displaced, may afterwards turn out to be very valuable. When Crookes started on a search for Newton's corpuscles by constructing a radiometer, he was generally laughed at and his motives explained away by the received hypotheses, but in passing electric discharges through glass tubes exhausted more perfectly than had been done before, he hit on the phenomena of radiant matter, which are now explained by corpuscles much smaller than the atoms.

Rankine was a frequent attendant at the meetings of the British Association, where his social gifts, added to his scientific eminence, made him a conspicuous figure. He was president of the section of engineering, and also of the section of mathematics and physics; and rose to be "King" of the social section known as Red Lions. At the meeting held at Bath in 1864 he produced "The Three-foot Rule," a song about standards of measure, and sang it, to his own accompaniment and in the capacity of a British workman.

When I was bound apprentice, and learned to use my hands,
Folk never talked of measures that came from foreign lands;
Now I'm a British Workman, too old to go to school,
So whether the chisel or file I hold, I'll stick to my three-foot rule.

Some talk of millimeters, and some of kilograms,
And some of deciliters, to measure beer and drams;
But I'm a British Workman, too old to go to school,
So by pounds I'll eat, and by quarts I'll drink, and I'll work by my
three-foot rule.

A party of astronomers went measuring of the Earth,
And forty million meters they took to be its girth;
Five hundred million inches, tho', go through from pole to pole;
So let's stick to inches, feet and yards, and the good old three-foot rule.

The great Egyptian pyramid's a thousand yards about;
And when the masons finished it, they raised a joyful shout;
The chap that planned that building, I'm bound he was no fool,
And now 'tis proved beyond a doubt he used a three-foot rule.

Here's a health to every learned man, that goes by common sense,
And would not plague the workman by any vain pretence;
But as for those philanthropists who'd send us back to school,
Oh! bless their eyes, if ever they tries to put down the three-foot rule.

This song indicates the great inconvenience and expense which would for a short time follow the change to the metric system; but it says nothing of the enormous inconvenience and expense which must always accompany the continued use of that muddle of units which prevails in Great Britain, and to a lesser degree in the United States. The want of system in the units obscures and clouds the whole subject of arithmetic; the school boy's time is spent on artificial reductions instead of the real relations existing between quantities. Consider the great convenience of the American decimal system of coinage, compared with the pounds, shillings, pence and farthings still inflicted on commerce in the old country. It was learned men of the three-foot rule type who prevented the decimal reform of the coinage advocated by De Morgan. " Too old to go to school " is a sentiment worthy of the Chinese, and its prevalence in Great Britain for generations is a cause which at the present moment threatens her industrial supremacy. The argument drawn from the length of the polar axis of the Earth, is said to be due to Sir John Herschel. At one time it was possible to choose the yard and the pound, but that time has been allowed to slip away. The system of electric units, universally adopted, calls for a change to the meter and the kilogram. Had Rankine received any part of his education abroad, he would probably have opposed this *insular* idea; his

colleague, Sir William Thomson was so educated, and has all his life been an enthusiastic advocate of the metric system.

When one sails up the river Clyde towards Glasgow, he sees on either bank a long succession of shipbuilding yards. Glasgow was in Rankine's time famous for its naval architects and shipbuilders, and they were Rankine's special friends. Hence, he was led into a number of investigations which are of importance in navigation. One of his papers is on the exact form of waves near the surface of deep water, and another investigates the lines of motion of water flowing past a ship. M. Napier, a naval architect, asked him to estimate the horse-power necessary to propel at a given rate a vessel which he was about to construct; and supplied him in confidence with the results of a great number of experiments on the horsepower required to propel steamships of various sizes and figures at various speeds. Rankine deduced a general formula, which he communicated to Napier directly and to the world at large in the form of an anagram: 20A. 4B. 6C. 9D. 33E. 8F. 4G. 16H. 10I. 5L. 3M. 15N. 14O. 4P. 3Q. 14R. 13S. 25T. 4U. 2V. 2W. 1X. 4Y.

The meaning of this anagram was afterwards explained as follows: "The resistance of a sharp-ended ship exceeds the resistance of a current of water of the same velocity in a channel of the same length and mean girth, by a quantity proportional to the square of the greatest breadth, divided by the square of the length of the bow and stern." Rankine and his naval friends prepared an elaborate *Treatise on Shipbuilding* which was published in 1866.

Rankine's only brother had died while yet young, and it seems that in later life his father and mother lived with him. In 1870 his father died, and in the following year his mother. Rankine never married; when he composed the song about a bridal tour to the Carrick Hills, his eyesight had failed so that he could not read. He had undertaken to write the memoir of John Elder, a shipbuilder, and this he was able to finish in 1872. Mrs. Elder endowed his chair so that it is now called the John Elder Chair of Engineering; it was however too

late to benefit Rankine. A substitute had to be appointed to take charge of his classwork; and at the close of the year he died suddenly; not of any special disease, but as the result of overwork. His death occurred on the 24th of December, 1872, in the 53d year of his age.

When I first came to this country and attended a meeting of the American Association for the Advancement of Science, I was eagerly sought out by a professor of the Stevens Institute who was a great admirer of Rankine and desired to learn about his personality. I had to say that I had never met Rankine but that he could learn something of the man from the *Collection* of his Songs and Fables. The fables are founded on the curious signs which distinguish inns in England, such as the "Swan with two necks," the "Cat and Fiddle," etc.; they are illustrated by a lady who was a cousin of Maxwell and who also depicted scenes in Maxwell's country life. From my conversation with this professor I learned how widely the engineering manuals of Rankine were used in the United States, that his thermodynamic researches were well known, and that his name was everywhere held in high honor.

# PETER GUTHRIE TAIT *

## (1831–1901)

Peter Guthrie Tait was born at Dalkeith, near Edinburgh, Scotland, on the 28th of April, 1831. His father was then private secretary to the Duke of Buccleuch, afterwards, I believe, a bookseller and the publisher of a monthly called *Tait's Magazine.* Peter Guthrie was educated at the Grammar School of Dalkeith, then at the Circus Place School in Edinburgh, and eventually at the Edinburgh Academy, where he had Maxwell for a classmate. Of equal age and similar genius they were drawn into close friendship. They left the Academy together, and took up the same classes of mathematics and physics at the University of Edinburgh. But while Maxwell continued in his studies there for three years, and drank deeply of philosophy and natural science as well as of mathematics and physics, Tait left after one brief session for the University of Cambridge. I dare say had Tait studied philosophy and natural science as Maxwell did, his writings would have been more logical, and his mental makeup less eccentric.

When he entered Peterhouse College, Cambridge, he was 18 years of age. His private tutor was William Hopkins the most successful coach of his time. He graduated as senior wrangler in 1852, and was also first Smith's prizeman. He was immediately made a mathematical tutor to his college, and very soon a Fellow. The second wrangler and second Smith's prizeman of the same year was W. J. Steele, an intimate friend of Tait. The two friends proceeded forthwith to prepare in conjunction a treatise called *Dynamics of a Particle;* but Steele lived to write only a few chapters. The book was

* This Lecture was delivered on March 22, 1902.—Editors.

first published in 1856, and has gone through a number of editions, Steele's name remaining on the title page. Two years after graduation he was appointed professor of mathematics in the Queen's College, Belfast. Then, if not before, he became acquainted with Andrews, the professor of chemistry and vice-president of the college; a skillful experimenter, famous for his researches on the nature of ozone and on the compression of gases. It is doubtful whether Tait did any experimenting under Forbes at Edinburgh; Andrews appears to have been his guide and master in physical manipulation.

In 1853, one year after Tait's appointment at Belfast, Hamilton published his *Lectures on Quaternions.* The young professor had a great power of doing work; in the day time he taught mathematics and experimented with Andrews; and at night he studied the new method of Quaternions. He soon mastered it sufficiently to be able to write papers on it, which he published in the *Messenger of Mathematics* and the *Quarterly Journal of Mathematics* and eventually he planned a volume of examples on Quaternions. There were, however, to Tait's mind numerous obscure points in the theory, and to elucidate them he wished to correspond with Hamilton directly. His friend Andrews wrote to Hamilton asking the favor; in this way a correspondence originated which was kept up till the death of Hamilton. In 1859 Hamilton met Tait at the British Association meeting at Aberdeen, and Tait introduced another disciple, Clerk Maxwell, then professor of physics at Aberdeen. The year following Professor Forbes resigned the chair of physics in Edinburgh University; the former schoolmates, Tait and Maxwell, were both candidates; the choice of the electors fell on the energetic professor of mathematics at Belfast. This contest, it is pleasant to say, did not diminish the friendship between the two mathematicians. In his letter Tait used the symbol $\frac{dp}{dt}$ for Maxwell, because in thermodynamics there is the equation $\frac{dp}{dt} = \text{J.C.M.}$ Maxwell addressed one of his odes to Tait as "The chief musician upon Nabla."

Tait's Quaternion project had now developed into a formal introduction to Quaternions; an announcement of the forthcoming book appeared soon after Tait removed to Edinburgh. He had now ceased to teach pure mathematics; he and Prof. William Thomson had sketched out an elaborate treatise on natural philosophy in four volumes; for which reasons he was anxious to have the Quaternion volume off his hands. Sir William Hamilton was then engaged in the preparation of his "*Elements of Quaternions*," and he did not like the idea of Tait's book appearing before his own. He did not object to examples, but he wished to have the priority in all matters of principle. Tait, hearing of the situation, offered of his own accord to delay the publication of his volume until the Hamilton's *Elements* should have appeared. To arrange the matter more definitely Tait made a visit to Dunsink Observatory, Dublin, in the summer of 1861. Hamilton expected to publish before the end of the year, and asked Tait to wait till the year following. But the printing of Hamilton's book went on for four years longer, and was stopped only by Hamilton's death in 1865. It was published, incomplete, in 1866; and true to his promise Tait did not publish till 1867. The work then given to the public was entitled an *Elementary Treatise on Quaternions*. The articles which deal with the theory of Quaternions have always presented numerous difficulties to the reader; this phenomenon is explained partly by the history of the volume, and especially by Hamilton's desire that Tait should confine the work to applications. I think it unfortunate that Hamilton adopted such an attitude. It was a mistake to present the method in such tremendous volumes as the *Lectures* and the *Elements*; it was a mistake to retard the publication of Tait's volume; it was a mistake to reserve the discussion of principles and of notation. Unfortunately, Tait, in his turn, advised inquirers to leave principles and notation alone and go on to applications, from which it has come about that the method of quaternions, presenting as it does, many points of novelty to the mathematician, has never been adequately discussed; only a few have looked upon it as a very important subject for discussion.

When Tait became professor of physics at Edinburgh University, laboratory teaching of physics was unknown in Scotland. It had been Forbes' custom to allow now and then a promising pupil such as Maxwell the use of the lecture apparatus, and in this as in many other customs he was followed by Tait. About ten years later Prof. Tait, following the example of Prof. Sir William Thomson of Glasgow, instituted a practical class. It was his idea that each student, taking that class, should be instructed how to make a series of measurements, and then should try some real experimental problem. Prior to the founding of the Cavendish Laboratory at Cambridge, the facilities at Edinburgh and Glasgow for gaining an experimental knowledge of physics were the best in Great Britain and this was due to the circumstance that in these twin cities of the North, the chairs of physics were occupied by twin giants in physical science. At the Scottish Universities the academic classes meet only in the winter—for six months; the medical and other professional classes have a summer session in addition. In the winter session Tait lectured five times a week to the academic students, about 200 in number, and endeavored to traverse the whole range of elementary physics. Every other Saturday there was a one-hour examination; at which, following a custom of his predecessor, he did not give out printed questions, nor write them on a board, but dictated them at uniform intervals of five minutes. Having propounded his problem Tait grinned with satisfaction; if a member of the class asked a question about it Tait reminded him that he had changed for the time being from a benevolent teacher into a relentless inquisitor. The papers were afterwards returned marked with 1 or 0 or ——, the length of the dash indicating the degree of imperfection.

To help those who wished to make a more thorough study of physics he instituted an advanced class; at this work he appeared to the greatest advantage. Before entering the lecture-room he glanced for a short time at his notes; thereafter he would write out mathematical equations for an hour without referring to any notes whatever. It was astonishing to see

the way in which he could "sling" the symbols. Tait was not only an intellectual, but likewise a physical, giant. I am nearly six feet high, but standing beside Tait, I used to feel diminutive. He was well-built, and muscular. He wore a long beard, the hair on the top of his head had disappeared at an early date, and left exposed a massive forehead. To protect his head while lecturing it was his custom to wear a skull cap. On the street he wore a sack-coat and a soft felt hat, and with cane in hand, was always walking rapidly. About the time of his moving to Edinburgh he married a lady who proved a genuine helpmate. She took full charge of all the affairs of the household, so that her distinguished husband might have perfect leisure for his scientific labors; and her influence was also such as to steady his attachment to religion.

Before the year 1860, when Tait became a professor of physics, Joule had made his determination of the mechanical equivalent of heat, thus establishing the first law of thermodynamics; Thomson, Rankine and Clausius had established the second law; and Rankine had drawn the outlines of the science of "energetics." In the first edition of *Dynamics of a Particle* there is no mention of the doctrines of energy; it is probable that Tait's experimental work with Andrews led him to study the papers of Thomson, Joule and Rankine. Anyhow the main object of Thomson and Tait's *Treatise on Natural Philosophy* was to fill up Rankine's outlines,—expound all the branches of physics from the standpoint of the doctrine of energy. The plan contemplated four volumes; the printing of the first volume began in 1862 and was completed in 1867. The other three volumes never appeared. When a second edition was called for, the matter of the first volume was increased by a number of appendices and appeared as two separately bound parts. The volume which did appear, although judged rather difficult reading even by accomplished mathematicians, has achieved a great success. It has been translated in French and German; it has educated the new generation of mathematical physicists; and it has been styled the "Principia" of the nineteenth century. Such was his admiration of Newton

that Tait I am sure could not conceive of any higher compliment. Maxwell had facetiously referred to Thomson as T and to Tait as T¹. Hence the *Treatise on Natural Philosophy* came to be commonly referred to as *T and T*¹ in the conversation of mathematicians.

It appears that the introduction of the quaternion method was a serious point of difference between the joint authors. Prof. Thomson, as you know, subsequently became Lord Kelvin and recently he wrote to Prof. Chrystal as follows with respect to the joint authorship of the Treatise. "I first became personally acquainted with Tait a short time before he was elected professor in Edinburgh; but, I believe, not before he became a candidate for the chair. It must have been either before his election or very soon after it that we entered on the project of a joint treatise of natural philosophy. He was then strongly impressed with the fundamental importance of Joule's work, and was full of vivid interest in all that he had learned from and worked at, with Andrews. We incessantly talked over the mode of dealing with energy which we adopted in the book, and we went most cordially together in the whole affair. He gave me a free hand in respect to names, and warmly welcomed nearly all of them. We have had a thirty-eight years' war over quaternions. He had been captivated by the originality and extraordinary beauty of Hamilton's genius in this respect, and had accepted, I believe, definitely, from Hamilton to take charge of quaternions after his death, which he has most loyally executed. Times without number I offered to let quaternions into Thomson and Tait, if he could only show that in any case our work would be helped by their use. You will see that from beginning to end they were never introduced."

In 1864 Tait published in the *North British Review* articles on "The dynamical theory of heat" and "Energy" which were afterwards made the basis of his *Sketch of Thermodynamics* published in 1868. The articles, mainly historical, are written from the British point of view, so much so, that he was accused of Chauvinism. To this charge he replied, "I cannot pretend

to absolute accuracy, but I have taken every means of ensuring it, to the best of my ability, though it is possible that circumstances may have led me to regard the question from a somewhat too British point of view. But, even supposing this to be the case, it appears to me that unless contemporary history be written with some little partiality, it will be impossible for the future historian to compile from the works of the present day a complete and unbiased statement. Are not both judge and jury greatly assisted to a correct verdict by the avowedly partial statements of rival pleaders? If not, where is the use of counsel?"

A German physician named Mayer was struck by the amount of heat developed in the team of horses which pulled the stagecoach into his village; and he reflected on the connection between the amount of heat developed and the amount of work they had done. From this as a starting point he was led to investigate the nature of heat, and he arrived at the now accepted doctrine that heat is a motion of the small parts of bodies. He sought after the exact mechanical equivalent of heat, and was able to deduce it by calculation from determinations of the specific heat and some other properties of air. He had not the means for making any experiments. Tait pointed out defects in Mayer's reasoning, and minimized his contribution, because he had not made any experiments. Prof. von Helmholtz, in reply, pointed out that Mayer was not in a position to make experiments; that he was repulsed by the physicists with whom he was acquainted; that he could scarcely procure space for the publication of his paper; and that in consequence of these repulses his mind at last became affected. Tait felt that he had been taking a rather ungracious attitude towards one who had suffered much for the sake of truth in science.

It cannot be denied that Chauvinism was one of the eccentric characteristics of Prof. Tait. He had never studied on the Continent; he never traveled, I believe, beyond the narrow confines of the British Islands; and in his later years, he became something of a recluse. What he said of the life of Rankine, applied with still greater force to his own. "The life of a

genuine scientific man is, from the common point of view, almost always uneventful. Engrossed with the paramount claims of inquiries raised high above the domain of mere human passions, he is with difficulty tempted to come forward in political discussions even when they are of national importance, and he regards with surprise, if not with contempt, the petty municipal squabbles in which local notoriety is so eagerly sought. To him the discovery of a new law of nature, or even of a new experimental fact, or the invention of a novel mathematical method, no matter who has been the first to reach it, is an event of an order altogether different from, and higher than, those which are so profusely chronicled in the newspaper. It is something true and good forever, not a mere temporary outcome of craft or expediency. With few exceptions, such men pass through life unnoticed by, almost unknown to, the mass of even their educated countrymen. Yet it is they who, far more than any autocrats or statesmen, are really molding the history of the times to come. Man has been left entirely to himself in the struggle for creature comforts, as well as for the higher appliances which advance civilization; and it is to science, and not to so-called statecraft, that he must look for such things. Science can, and does, provide the means; statecraft can but more or less judiciously promote, regulate or forbid their use or abuse. One is the lavish and utterly unselfish furnisher of material good; the other the too often churlish and ignorant dispenser of it."

His next book was written in conjunction with Prof. Kelland, *An Introduction to Quaternions*, 1873. Kelland was the professor of mathematics, and it was his custom to expound to his senior class the elements of quaternions along with advanced algebra. Tait, so far as I know, never lectured on the subject at the University of Edinburgh. The volume in question grew out of Kelland's lectures, and was revised and supplemented by Tait. Kelland was much the older man, and had stood to Tait in the relation of instructor. In the preface, which was written by Kelland, light is thrown on the relation between the joint authors and colleagues: "The

preface I have written," Kelland says, " without consulting my colleagues, as I am thus enabled to say what could not otherwise have been said, that mathematicians owe a lasting debt of gratitude to Prof. Tait for the singleness of purpose and the self-denying zeal with which he has worked out the designs of his friend Sir William Hamilton, preferring always the claims of the science and of its founder to the assertion of his own power and originality in its development. For my own part I must confess that my knowledge of Quaternions is due exclusively to him. The first work of Sir William Hamilton—*Lectures on Quaternions*—was very dimly and imperfectly understood by me and I dare say by others, until Prof. Tait published his papers on the subject in the *Messenger of Mathematics*. Then, and not till then, did the science in all its simplicity develop itself to me."

Tait had now co-operated with Steele in writing *Dynamics of a Particle*, with Thomson in a *Treatise on Natural Philosophy*, and with Kelland in the *Introduction to Quaternions*. There was still a fourth literary partnership to follow; this time with Balfour Stewart, professor of physics at the Owens College, Manchester. In 1875 a volume called *The Unseen Universe*, having as a sub-title " Physical Speculations on a Future State " appeared anonymously; but to a physicist it was evidently inspired by Tait's *Sketch of Thermodynamics* and Stewart's book *The Conservation of Energy*. It was asserted in the *Academy* that Tait and Stewart were the authors; and a subsequent edition appeared with their names on the title page. It was to most people a matter of surprise that one who had been denouncing metaphysics in season and out of season, should turn out to be part author of a book described as " physical speculations on a future state." Did not Kant say that the three problems of metaphysics are God, freedom, and immortality? What is metaphysics but speculation based upon physical science concerning things which can never be reached directly by the methods of physics? *The Unseen Universe* was metaphysics of the best or worst (however you may view it) kind; it was full of Carnot's reversible engine, the

mechanical equivalent of heat, vortex-atoms and so forth. In subsequent editions, and there are many, the physical basis disappeared more and more; and the book took more of the appearance of a philosophical and theological essay.

In my lecture on Clifford * I explained how an anagram had appeared in *Nature* in 1874 and how that later the anagram was explained in *The Unseen Universe* as follows: "Thought conceived to affect the matter of another universe simultaneously with this may explain a future state." The kernel of the book is this so-called discovery. Preliminary chapters are devoted to a survey of the beliefs of ancient peoples about the immortality of the soul; to physical axioms, to an exposition of the doctrines and hypotheses concerning energy, matter and ether; and to the biological doctrine of development; it is only in the last chapter that we come to the "unseen universe." What is meant by the "unseen universe?" Matter, according to the authors is made up of molecules, which are supposed to be vortex-rings made of the luminiferous ether; the luminiferous ether is in turn supposed to be made of much smaller molecules which are vortex-rings of a second ether. These smaller molecules with the ether in which they float constitute the unseen universe. The authors see reason to believe that the unseen universe absorbs energy from the visible universe and *vice versa;* in this way a communication is established between them. The human soul is a frame made of the refined molecules and exists in the unseen universe, although in life it is attached to the body. Every thought we think is accompanied by certain motions of the coarse molecules of the brain; these motions are propagated through the visible universe, but a part of each motion is absorbed by the fine molecules of the soul. Consequently the soul as well as the body has an organ of memory; at death the soul with its organ of memory is simply set free from association with the coarse molecules of the body. In this way, the authors considered that they had shown the physical possibility of the immortality of the soul. So far the book may be considered to be a legitimate and inter-

* Ten British Mathematicians, p. 89.—EDITORS.

esting metaphysical speculation. But the authors proceeded further to apply their speculation, to explain the main doctrines of Christianity. Hypotheses about the nature of matter may change, and have changed wonderfully since 1875, and no one cares to see sacred truths placed on so precarious a foundation as the vortex theory of matter.

Such was the immediate success of the book, from the point of view of sales, that the authors were induced to venture on a novel *Paradoxical Philosophy: a Sequel to the Unseen Universe.* The hero is a Dr. Stoffkraft, who goes to Strathkelpie Castle to take part in an investigation of spiritualistic phenomena. He begins by detecting the mode in which one young lady performs her spirit-rapping, but forthwith falls into an "electro-biological" courtship of another, and, this proving successful, he is persuaded by his wife and her priest to renounce the black arts in the lump as works of the devil; and then settles down to compose an "Exposition of the Relations between Religion and Science," which he intends to be a thoroughly matured production. He advocates various materialistic views, but the other guests at the castle, who compose the Paradoxical Club, have read *The Unseen Universe*, and work discomfiture on Dr. Stoffkraft by arguments drawn from it.

About this time, 1876, Tait published a volume entitled *Lectures on Recent Advances in Physical Science.* These lectures, prepared at the request of a number of professional men, chiefly engineers, were delivered in the physics theater of the University. They were edited from the report of a stenographer, and they give a very good idea of Tait's style as a lecturer. He was in his time considered the finest lecturer of the Edinburgh University. On reading these lectures, published only twenty-five years ago, one is struck by the greatness of the advances made since, especially in the domain of electricity. In them there is no mention of the telephone or microphone, of the dynamo or incandescent lamp; electric waves and $X$-rays are yet undemonstrated. The advances treated of are the doctrine of energy, spectrum analysis, the conduction of heat, and the structure of matter. Prof. Tait was accustomed

to spend his vacation at the ancient city of St. Andrews, on the sea coast where there is a magnificent course for golf. On one occasion soon after these lectures were published both he and a Glasgow professor of theology, a metaphysician of the Hegelian school, were invited to a dinner in that city. Tait was very naturally drawn out to talk about the subjects on which he had been lecturing, and he did so largely and to the delight and edification of everyone except the Hegelian, who when he could stand it no longer, gravely put the question: " But, Mr. Tait, do you really mean to say that there is much value in such inquiries as you have been speaking about?" After that the subject was changed, and during the rest of the evening the mathematician and the metaphysician did little else than, as one of the company expressed it, " glour at each other."

We have seen that Tait attended the meeting of the British Association at Aberdeen in 1859; but he was not a frequent attendant, for he said that there was too much jabber and talk, and that he did not care for great " spreads." At one of the Edinburgh meetings (1871) he was president of the section of mathematics and physics, on which occasion he delivered an address on Hamilton's Calculus of Quaternions and Thomson's Principle of the Dissipation of Energy. When the Association met in Glasgow in 1876, he was requested on short notice to deliver one of the popular lectures. He took for his subject Force, he made a plea for the accurate use of terms in mechanical science, a reform which has progressed much since that time. He says that force, defined as the rate of change of momentum in a body is also the space-variation of potential energy. Another point he insisted on is that *matter* and *energy* are things—have objective existence, because their quantity in the universe is constant; *force* on the other hand cannot be a thing, or have objective existence, because its quantity is indeterminate. " It is only things," he said, " which can be sold." In view of this dictum it is interesting to observe that some courts have held that an electric current cannot be stolen, as it was not a thing. But what is stolen is the energy of the current, and according to Tait's ideas energy is a thing.

In the same lecture Tait gave a succinct statement of his philosophy of knowledge. "In dealing with physical science" he said, "it is absolutely necessary to keep well in view the all-important principle that *Nothing can be learned as to the physical world save by observation and experiment, or by mathematical deductions from data so obtained.* On such a text volumes might be written, but they are unnecessary, for the student of physical science feels at each successive stage of his progress more and more profound conviction of its truth. He must receive it, at starting, as the unanimous conclusion of all who have in a legitimate manner made true physical science the subject of their study, and, as he gradually gains knowledge by this—*the only*—method, he will see more and more clearly the absolute impotence of all so-called metaphysics or *a priori* reasoning, to help him to a single step in advance. Man has been left entirely to himself as regards the acquirement of physical knowledge. But he has been gifted with various *senses* (without which he could not even know that the physical world exists) and with *reason* to enable him to control and understand their indications. Reason, unaided by the senses, is totally helpless in such matters. The indications given by the senses, unless interpreted by reason, are utterly unmeaning. But when reason and the senses work harmoniously together, they open to us an absolutely illimitable prospect of mysteries to be explored."

What, it may be asked, is this *reason* which interprets the indications of the senses? Is it not the very *a priori* knowledge which the rational philosophers have ascribed to the mind? If so, why all this tirade against so-called metaphysics and *a priori* reasoning? To one who held that all knowledge came through the senses, such procedure would be logical, but not to the savant who uttered the above theory of knowledge. The speculations in the *Unseen Universe* assume the truth of the vortex theory of atoms. According to the ancient idea of the atom, it is a hard incompressible sphere. Boscovich removed the idea of hardness, and reduced the atom to a mere centre of force. Rankine, we have seen, supposed the point surrounded

by a vortex, whirling round an axis passing through the point. Helmholtz investigated the properties of a vortex-ring such as skillful smokers emit. The whirling is round the core of the ring, and is associated with a progressive motion. Thomson replaced Rankine's vortex-atmosphere with Helmholtz's vortex-ring; and showed that the properties of the vortex-ring in a perfect fluid would account for the indestructibility, elasticity and difference in kind of the atoms. The simplest kind of vortex is the unknotted ring. Suppose that one knot is put on the ring before the ends are tied; this will give the trefoil knot. It has three crossings, and was supposed to figure an essentially different kind of atom.

Professor Tait investigated all the essentially different forms up to nine crossings, and contributed his results to the Edinburgh Royal Society. "Clever," some said, "but what is the use of it." The application was obvious; to elaborate the vortex-ring theory of atoms. Since then, however, electrical investigations have thrown more light on the subject of the atoms, so that Lord Kelvin is for going back to Lucretius.

In the discharge of his duties as a teacher, Tait was a model to his colleagues. The lecture always began punctually at seven minutes past the hour, and did not end till the clock struck the next hour. Lecturing to undergraduate students he never obtruded his own researches, still less made them the subject of lectures; he had a conscientious desire to teach them thoroughly the appointed subjects. He was also punctual in his attendance at the laboratory. In the summer term he came about 11 o'clock, would discuss results and plans with the researchers, take up his own investigation, and generally leave about 2 o'clock. In those days the physical laboratory did not remain open for long hours—from 10 to 3. He had little liking for the general business of the University, and in later years he was to be found only in his lecture room, or laboratory at the University, in his library at home, or in the hall of the Edinburgh Royal Society. For many years he was general secretary, and did Herculean work for the Society. He never sought fellowship in other scientific societies, and the scientific

honors he received were not in proportion to the greatness of his scientific achievements.

In the summer time, after the close of the University session, it was Tait's invariable custom to spend the vacation on the links at St. Andrews. He was an enthusiastic golfer, and exemplified the harmony of theory and practice. He investigated by observation and experiment the various physical phenomena, the chief of which is the long time during which the golf ball remains in the air notwithstanding the slight elevation of its path above the ground. To investigate the path and velocity of the ball he made a drive and bunker in the basement of the University building. He communicated his results to the Royal Society of Edinburgh and there stated definitely the longest distance to which a golf ball could possibly be driven. One of his sons, Frederick Guthrie Tait, acquired great skill as a golfer. He was a lieutenant in the famous regiment called Gordon Highlanders, and also the champion amateur golfer of the British Islands. Such was his fame and prowess that to the general public Tait, the eminent mathematician, became known to them from being the father of the champion golfer. Prof. Tait enjoyed his son's success immensely for the buoyant and sanguine temperament of youth remained his throughout life. But the champion golfer upset his father's calculations of the greatest possible distance by driving a ball five yards further!

In the course of his long career Tait was engaged in many polemical discussions. Look over the columns of *Nature*, and you will find controversies with Tyndall, Proctor, Zöllner, Poincaré, Gibbs, Heaviside and many others. He was apt to take an exaggerated view of men—Newton was nothing short of a god, Leibnitz nothing better than a devil; whereas the truth is that Newton and Leibnitz were both men of many virtues but also of some failings. Tait himself was a man of many heroic virtues, mixed with a few inconsistencies. In these polemical discussions he used exaggerated language, which was probably taken more seriously than he intended. Anyhow a stranger introduced to him in his retiring room at

the University, found a very genial and buoyant gentleman, very different from any idea imagined from reading his controversial letters. As regards those who attended his lectures, he commanded their respect and admiration, while the attitude of his research students can be expressed only by veneration and love.

In 1897 his health began to break down before the end of the arduous winter session; but it was recuperated by a vacation on the links at St. Andrews. He had a splendid physique; but it had long been his custom to remain in his library to very late hours, reading, or writing at a plain wooden desk (which he did standing); these long hours of study and mental work eventually told upon his health.

Lieutenant Tait, the champion golfer, was ordered with his regiment to the field of action in South Africa. His regiment (the Black Watch) suffered heavily in the engagements at the Modder River, directed by the unfortunate Lord Methuen. It was reported that Lieutenant Tait had been killed, but his fate remained uncertain for six weeks. He was killed at Koodoosberg, where a white cross now marks his grave. The story of his life has appeared in a book *F. G. Tait: a record.* The loss was a serious blow to Prof. Tait, already in failing health. Early last year he was unable to attend to any of the duties of his chair, and he sent in his resignation. It was hoped that, freed from teaching duties, his health might recover. At the beginning of July, 1901, he went to the seashore near Edinburgh to spend some days at the house of his friend Sir John Murray, editor of the "Challenger" reports. On July 4, he spent the afternoon in the garden and filled a sheet of foolscap with a quaternion investigation; in the evening he suddenly became ill and died in the course of a few hours, aged 70 years and one month.

Before his death two volumes of his *Collected Works* had been published and a third will follow. At the time of his retirement those who had been trained by him in research took steps to prepare an illuminated address, but as they were scattered over all the world this was not fully finished at the time of his death, and it was presented to his widow. The address

is surrounded by designs emblematic of his principal labors; there is a scroll on which are inscribed certain quaternion equations, a portrait of Newton, a thermo-electric diagram, a deep-sea thermometer, a Crookes' radiometer, and a profusion of knots. There are 63 signatures to the address which reads as follows:

" Dear Professor Tait: We need hardly tell you how deeply we share the universal feeling of regret with which the announcement of your resignation of the chair of Natural Philosophy in the University of Edinburgh has been received. Your tenure of the chair has extended over a most momentous period in the advance of knowledge; and no small part of the progress of physical science, which has been so characteristic of that period, has been the result of your own work. By your investigations and writings you have placed the whole scientific world in your debt, and have added prestige to a chair already rendered illustrious by your distinguished predecessors. The many thousands who have gained from your direct personal teaching a real insight with the processes of nature, and a training in accuracy of thought and of language, will always recall with pleasure and pride that you were their teacher. We whose privilege it was to come into closer touch with you in classroom or in laboratory, have had our life-work in many cases determined and in all cases influenced by the inspiration and guidance received there; and no words can fully express the feelings of reverence and affection which we entertain towards you. Yet, however feeble the expression, we ask you to accept it as our tribute of appreciation and of gratitude for all you have been to us as an intellectual stimulus and as a moral force. Your retirement is an irreparable loss to the University; but if, by relieving you from the arduous duties of the chair, it enables you to devote yourself more entirely to investigation and research, the world will without doubt have the greater gain. We wish you many years of health and strength both for the enjoyment of a well-earned leisure and for the further exercise of an unusually fruitful scientific activity."

But it was not to be.

# SIR WILLIAM THOMSON, FIRST LORD KELVIN *

(1824–1907)

William Thomson, now Lord Kelvin, was born in Belfast, Ireland, on the 26th of June, 1824. He is of Scottish-Irish descent. His father was James Thomson, then professor of mathematics in the Royal Belfast Academical Institution, who had a remarkable career; he was descended from a family of Thomsons, who had for several generations occupied a farm near Ballynahinch, County Down, in the north of Ireland. When a boy he endeavored all alone to understand the principles of drilling and in this way was led to study mathematics. As a result he was sent to a small grammar school in his native place, where he rose to be an assistant teacher. Soon he became able to attend the University of Glasgow during the winter session, by teaching in the local school during the summer.

After studying in this manner for five years he was appointed to a position in the Belfast Institution mentioned, where he was promoted to the professorship of mathematics. He was the author of an algebra, which was popular with teachers for many years, and his reputation was such that in 1832 he was appointed professor of mathematics in the University of Glasgow.

William Thomson was then six years old, and his brother James Thomson nearly two years older. They were educated together at home by their father, and in 1834 they became students together at the University of Glasgow. At the Scottish Universities the conditions for entrance, were then, and still are, rather loose—no inferior limit to the age, and no entrance examinations to pass. William Thomson, when he entered, was but little over ten years of age. He studied for six years.

* This Lecture was delivered on March 25, 1902.—Editors.

but did not take a regular course leading to a degree. He had the genius of a mathematician, and his father was not slow to discover it. Accordingly he was sent when seventeen years of age to the University of Cambridge, where he became a student of St. Peter's College, the oldest foundation of that University (600 years). His undergraduate career at Cambridge extended over four years. In his first year he contributed a paper signed P. Q. R., to the Cambridge *Mathematical Journal*, in which he defended Fourier's *Treatise on Heat* from some criticisms made by Prof. Kelland of Edinburgh University. This paper was followed in the same journal by two others of still greater importance: "The uniform motion of heat in homogeneous solid bodies and its connection with the mathematical theory of Electricity" and "The linear motion of heat." In the former paper he points out the analogy between the theory of the conduction of heat in solid bodies and the theory of electric and magnetic attraction; and pursuing this analogy he makes use of known theorems about the conduction of heat to establish some of the most important theorems in the mathematical theory of electricity. The latter paper contains the foundations of the method which he afterwards applied to find limits to the age of the Earth. In his undergraduate career Thomson was well-known for his skill in boating; he was also president of the musical society. Probably he did not, as much as his rivals, concentrate his attention on the subjects which would pay in the final examinations; anyhow, he came out second wrangler. Although he was unsuccessful in the struggle for supremacy as determined by the blind adding of marks, one of the examiners declared that the senior wrangler was not fit to cut pencils for Thomson. In the subsequent more scientific test—the competition for the Smith prizes—he obtained the first place. He was immediately elected a Fellow of his college.

At this time (1845) the Analytical Society founded by Peacock, Herschel, and Babbage had accomplished its reform. But in Newton's University experimental investigation in physics had died out, the greatest mathematical physicists of the

day were in Paris—Fourier, Fresnel, Ampère, Biot, Regnault. So William Thomson went to Paris, and worked for a year in Regnault's laboratory, where classical determinations of physical constants were being made. Partly as a consequence of this step, Thomson has always been very popular with the scientists of France. When resident in Paris he published in Lionville's *Journal* a paper on the "Elementary Laws of Statical Electricity," in which he examined the experiments and deductions of Sir. W. Snow-Harris. This investigator had made an experimental examination of the fundamental laws of electric attraction and repulsion, and his results were supposed to disprove tne well-known simple laws of Coulomb. Thomson showed by pointing out the defects of Harris' electrometers that the results, instead of disproving these laws, actually confirmed them, so far as they went. From this examination dates Thomson's interest in electrometers, which led to the invention of the quadrant electrometer, the portable electrometer, and the absolute electrometer.

In 1846 the chair of Natural Philosophy in the University of Glasgow became vacant, and William Thomson was appointed at the early age of 22. I have heard it said that in the matter of appointments at Glasgow the principle of nepotism was powerful; in this case it was fortunate. Thomson's father was still the professor of mathematics, and remained so for three years longer; his brother, James Thomson, a few years later, became professor of engineering. At the same time Thomson was made editor of the Cambridge and Dublin *Mathematical Journal* (hitherto the Cambridge *Journal*). Among the contributors who supported him in this enterprise were Stokes, Cayley, Sylvester, De Morgan, Boole, Salmon, Hamilton, of whom only two now survive—Sir George Stokes, and Rev. George Salmon, Provost of Trinity College, Dublin.

While Thomson was a student at Cambridge, Joule made his investigations which determined the dynamical equivalent of heat. Thomson had made a special study of Fourier's *Treatise on Heat*, and had begun to apply his methods; consequently, on his return to Glasgow it was not long before he took up the

dynamical theory of heat. His first contribution, read before the Royal Society of Edinburgh in 1849, was a critical account of Carnot's memoir "Réflexions sur la puissance motive du feu." Joule's measurements were at first almost ridiculed, and had few hearty supporters; but one of these was Thomson. Carnot's theory of the heat-engine assumed that heat is a species of matter; Thomson set to himself the task to modify the theory to suit the doctrine that heat consists in the motion of the small particles of a body. His great stumbling block in the way of accepting the dynamical theory of heat was the difficulty of accurately defining temperature. Founding on Carnot's work Prof. Thomson put this matter upon a perfectly satisfactory scientific basis. Before he propounded his absolute scale of temperature, purely empirical scales founded on the behavior of various gases, liquids, and solids, had each its advocate, and there seemed to be no satisfactory reason for preferring one to another. Once he propounded the absolute scale, no question has ever since been raised but that it is the only rational scale to adopt as the absolute one. To carry out this idea he made experimental investigations in conjunction with Joule on the thermodynamic properties of air and other gases, and as a result showed how to define a thermodynamic scale temperature having the convenient property that air thermometers and other gas thermometers agree with it as closely as they agree with one another.

His thermodynamic investigations led to the doctrine of the dissipation of energy announced by him in 1852. "During any transformation of energy of one form into energy of another form there is always a certain amount of energy rendered unavailable for further useful application. No known process in nature is exactly reversible, that is to say, there is no known process by which we can convert a given amount of energy of one form into energy of another form, and then, reversing the process, reconvert the energy of the second form thus obtained into the original quantity of energy of the first form. In fact, during any transformation of energy from one form into another, there is always a certain portion of the energy changed into

heat in the process of conversion, and the heat thus produced becomes dissipated and diffused by radiation and conduction. Consequently, there is a tendency in nature for all the energy in the universe of whatever kind, gradually to assume the form of heat, and having done so, to become equally diffused. Now, were all the energy of the universe converted into uniformly diffused heat, it would cease to be available for producing mechanical effect, since for that purpose we must have a hot source and a cooler condenser. This gradual degradation of energy is perpetually going on; and, sooner or later, unless there be some restorative power, of which we at present have no knowledge whatever, the present state of things must come to an end." Maxwell imagined a restorative process which might be applied by intelligent demons. Suppose a portion of gas to be confined in a closed space, it will have a uniformly diffused temperature. Suppose a partition stretched across with a little door guarded by an intelligent demon. The molecules by their impacts and collisions really have different velocities; what is uniform is the mean velocity. If the demon in charge opens the door so as to let the swift molecules in B go into A, and the slow molecules in A go into B, the degradation of the temperature will be gradually restored.

In 1852 he was married to Miss Margaret Crum, daughter of Walter Crum, Esq. of Thomliebank; a devout lady much attached to the Presbyterian Church. As a consequence, he resigned his fellowship in St. Peter's College; but he was afterwards made an honorary fellow. About this time he organized the first physical laboratory in Great Britain. He had an abundance of experimental problems for his students to tackle particularly on the properties of metals. About four years after Thomson located at Glasgow, submarine telegraphy became an object of practical science. In the working of a submarine cable between England and Holland, it was observed that the signals were more difficult to receive than those from the end of an aerial line. Faraday was the first to investigate the cause of this overlapping of the signals. At first there was a great deal of confusion; speed of signaling was mixed up with velocity

of transmission; the duration of the signal was not distinguished from the time required to traverse the cables. Thomson investigated the phenomenon, and found that it was due to the capacity of the cable; and he deduced the practical result that with cables of equal lateral dimensions the retardations are proportional to the squares of the lengths. This law became known generally as the "law of squares." A Mr. Whitehouse, experimenting with a cable 1125 miles in length, found that the maximum effect of a signal communicated instantaneously at one end was received at the farther end in one second and a half. Applied to these data the "law of squares" said that as the distance from Ireland to Newfoundland is twice the length of the experimental cable, the time in which a signal communicated instantaneously would be received at the further end is $2.5^2$ seconds: that is, six seconds. It became evident that if only five signals could be sent in a minute, the financial success of an Atlantic cable was very doubtful, so Whitehouse fought manfully against the "law of squares." He said, "I can only regard it as a fiction of the schools, a forced and violent adaptation of a principle in physics, good and true under other circumstances but misapplied here." He also made experiments and published results which seemed entirely opposed to the law. To this Prof. Thomson replied in the *Athenæum* newspaper (Nov. 1, 1856), reiterating the application of the "law of squares" to submarine telegraphy, and showing that the experiments cited really confirmed the law they were supposed to disprove. He further maintained that, notwithstanding the law of squares, Atlantic telegraphy was possible, and stated his conviction that increase of the electric pressure was a development in the wrong direction. Prof. Thomson showed that the condition for rapid signaling consisted in being able to observe the first beginning of the electric current at the far end, and to stop the signal as soon as it had risen to this observable value. To realize these conditions he invented the delicate reflecting galvanometer in which the minute turning of the magnet is magnified by the motion of a spot of light. Maxwell wrote a parody on Tenny-

son's " Blow, bugle, blow," and called it " A Lecture to a Lady on Thomson's Reflecting Galvanometer ":

The lamplight falls on blackened walls,
And streams through narrow perforations,
The long beam trails o'er pasteboard scales
With slow-decaying oscillations—
Flow, current, flow, set the quick light-spot flying,
Flow current, answer light-spot, flashing, quivering, dying.

O look! how queer! how thin and clear,
And thinner, clearer, sharper growing
The gliding fire! with central wire,
The fine degrees distinctly showing.
Swing, magnet, swing, advancing and receding,
Swing magnet! Answer dearest, "What's your final reading?"

O love! you fail to read the scale
Correct to tenths of a division.
To mirror heaven those eyes were given,
And not for methods of precision—
Break, contact, break, set the free light-spot flying;
Break contact, rest thee magnet, swinging, creeping, dying.

In the above verses Maxwell describes the process of taking a quantitative reading for the amount of a steady electric current; for signaling, all that is necessary is to observe the direction towards which the spot of light is going to move. It was by the reflecting galvanometer that the historic message through the first Atlantic cable was received: " Europe and America are united by telegraphic communication. Glory to God in the highest, on earth peace and goodwill towards men."

Prof. Thomson was personally engaged in the laying of the first cable. It transmitted several messages, then stopped. It served to *prove* the feasibility of the project which many engineers up to that time regarded as chimerical. By the labors of Thomson, Varley, Jenkin and others the construction of the cable was improved, as well as the mechanical means for laying it, and in 1866 a new cable was successfully laid, and the old one of the previous year raised from the depths and

repaired. On his return from this labor in 1866, Prof. Thomson along with others of his distinguished coadjutors, received the honor of knighthood. Subsequently he invented a recording receiver for long cables, called the siphon recorder. We have seen that in 1860 Thomson and Tait entered upon the preparation of their treatise on natural philosophy, which was planned to extend to four volumes, but of which the first and last appeared in 1867. In this interval of years Thomson was likewise engaged on the Atlantic Cable, and in writing several cosmological papers, which have ever since been famous subjects for discussion: they were on the age of the Sun, the physical state of the interior of the Earth, and the age of the Earth as an abode for life.

The last mentioned subject was treated of in a paper " On the secular cooling of the Earth," read before the Royal Society of Edinburgh in 1862. He introduced the subject as follows: " For eighteen years it has pressed on my mind, that essential principles of thermodynamics have been overlooked by those geologists who uncompromisingly oppose all paroxysmal hypotheses, and maintain not only that we have examples now before us on the Earth, of all the different actions by which its crust has been modified in geological history, but that these actions have never, or have not on the whole, been more violent in past time than they are at present. It is quite certain the solar system cannot have gone on, even as at present, for a few hundred thousand, or a few million years, without the irrevocable loss (by dissipation, not by annihilation) of a very considerable proportion of the entire energy initially in store for Sun heat, and for Plutonic action. It is quite certain that the whole store of energy in the solar system has been greater in all past time than at present; but it is conceivable that the rate at which it has been drawn upon and dissipated, whether by solar radiation, or by volcanic in the Earth or other dark bodies of the system, may have been nearly equable, or may even have been less rapid, in certain periods of the past. But it is far more probable that the secular rate of dissipation has been in some direct proportion to the total amount of energy

in store at any time after the commencement of the present order of things, and has been therefore very slowly diminishing from age to age. I have endeavored to prove this for the Sun's heat, in an article recently published in *Macmillan's Magazine* (March, 1862), where I have shown that most probably the Sun was sensibly hotter a million years ago than he is now. Hence, geological speculation, assuming somewhat greater extremes of heat, more violent storms and floods, more luxuriant vegetation, and harder and coarser grained plants and animals, in remote antiquity, are more probable than those of the extreme quietist, or "uniformitarian" school. A middle path, not generally safest in scientific speculation, seems to be so in this case. It is probable that hypotheses of grand catastrophes destroying all life from the Earth, and ruining its whole surface at once, are greatly in error; it is impossible that hypotheses assuming an equability of sun and storms for 1,000,000 years can be wholly true."

He proceeded in the paper cited, to apply Fourier's results to deduce a limit to the age of the Earth. Suppose a solid slab of uniform thickness and of great lateral dimensions to be originally heated to a temperature $V°$, one side to be kept exposed to a temperature $O°$, and the other to be kept exposed to a temperature $V$. Let $k$ denote the conductivity of the solid, when measured in terms of the thermal capacity of the unit of volume; and let $v$ denote the temperature at any distance $x$ from the surface at any time $t$ from the beginning of the cooling. Fourier showed that under these conditions,

$$\frac{dv}{dx}=\frac{V}{\sqrt{\pi kt}}e^{-x^2/4kt}$$

Here $\frac{dv}{dx}$ means the gradient of temperature, along a line normal to the face; it is the rate of change of the temperature as you go along the direction of $x$. This formula does not apply to any time prior to the beginning of the cooling, for then $t$ will be negative and the formula involves the square root of $t$.

But what application has this result to the case of the Earth?

No doubt there still are people who think that the Earth is an infinite slab; but if the investigation has any application, it is to a solid globe, originally at one uniform temperature, exposed to a cooling agent at the surface. But the case of the Earth is reduced to the simple case of the slab by the following considerations. It had been ascertained by the observations of Forbes on underground temperature that change of temperature due to day and night, or summer and winter, disappears at about 24 feet below the surface; and observations in coalpits and borings show that the temperature thereafter increases at the rate of about one degree Fahrenheit per 50 feet of descent; but Fourier's results show that this rate will practically vanish at a small depth compared with the distance to the Earth's centre. Hence a spherical plate of the Earth if such thickness may be treated as a plate of the kind specified. The best value of $k$ then known was 400; hence for the case of the Earth,

$$\frac{dv}{dx}=\frac{V}{35.4\sqrt{t}}e^{-x^2/1600t}.$$

When $t$ is very large and $x$ small, the exponential factor is negligible; and we know that then $\frac{dv}{dx}$ is $\frac{1}{50}$; hence

$$\frac{1}{50}=V\frac{1}{35.4\sqrt{t}},$$

and

$$\sqrt{t}=V\frac{50}{35.4}.$$

Suppose $V$, the original temperature of the Earth, when it had just solidified, to be 7000° F., the temperature of melting rock, then $t \doteq 98,000,000$ years.

Prof. Thomson concluded that the age of the Earth as a possible abode for life must lie between 400,000,000 years and 20,000,000 years. These results came like a bolt from the blue sky on the geologists and biologists of the day. The former supposed that

physical changes went on in the past at the slow rate at which they take place now; and by a simple application of the rule of three, to the sedimentary rocks, demanded as much time as the above for a small portion of the secondary period. In the Earth they discovered no trace of a beginning, no indication of an end; and some of them, leaving the solid crust of the Earth, and looking out into the Universe could see no signs of age or decay in the solar system. The biologists too were explaining the evolution of forms by unlimited amounts of time. The great Darwin spoke of the proposed limitation of geological time as one of his " sorest troubles." It was indeed inevitable that a clash should come.

Four years later, 1866, Sir William Thomson read another paper to the Edinburgh Society, " The doctrine of uniformity in geology briefly refuted." It contained only a few sentences and was a formal indictment of the fundamental doctrine of the geologists. The geologists put up Prof. Huxley to defend them; which he did in an address to the Geological Society of London in 1869. We have seen in a previous lectuie how much Huxley knew of the nature of mathematics; he was scared at a few italic letters, particularly if they were small, not to mention the more formidable $\frac{dv}{dx}$. He could not discuss Thomson's arguments scientifically, all he could do was to make fun of them, and encourage his colleagues in their indifference. He said, as an introduction, " I do not suppose that at the present day any geologist would be found to maintain absolute uniformitarianism, to deny that the rapidity of the rotation of the Earth *may* be diminishing, that the Sun *may* be waxing dim, or that the Earth itself *may* be cooling. Most of us, I suspect, are Gallios, ' who care for none of these things,' being of opinion that, true or fictitious they have made no practical difference to the Earth, during the period of which a record is preserved in stratified deposits." If researches which are the outcome of dynamical reasoning, combined with observational and experimental data, applied to determine the constancy of the length of the day, the intensity of sunshine in different

ages, the age and temperature of the Earth are not *geology*, it is difficult to adduce anything which has a right to that title. Yet Huxley in the name of the geologists said that they were intellectual Gallios, caring for none of these things. It is certainly a very remarkable fact that one who fought all his life against ecclesiastical Gallios as regards evolution should, in the matter of the application of physical science to a geological problem, borrow their precise attitude and maxims.

The controversy has gone on ever since, and has enlivened many a meeting of the British Association. The geologists say to Lord Kelvin "Look at our arguments." Lord Kelvin says to the geologists "Look at mine." The former call out "cosmogonist"; the latter replied "geological calculus." As a result of the controversy the uniformitarian doctrine has disappeared; but no agreement has been reached about the age of the Earth when it became an abode for life. Kelvin's reasoning can be attacked only by questioning the values which are assumed for the constants, or by denying the conditions which are assumed to be true in applying Fourier's problem to the case of the Earth. The former course was adopted a few years ago by Prof. Perry; by modifying the constant $k$ he increased the time about tenfold. It is the only course which presents any avenue of escape such as the geologists desire to see. Is the Earth a body which was once molten hot, and has been subsequently left to cool, without any further generation of heat in the interior by oxidation of its contents? If the geologists had more mathematical training, they might be able to make better use of their data. As it is their reasoning is too much of this character: the Mississippi now carries down so much mud in a year, how long will it take at this rate to reduce the whole valley to the level of the Gulf of Mexico? This is a specimen of "logical calculus." A very slight knowledge of mathematics suffices, however, to show that natural changes take place at a variable rate which depends at any time on the amount to be changed, and until one gets a clear idea of a logarithm and an exponential he will not be able

to reason to much purpose on the time required for any of the works of Nature.*

Sir William Thomson's labors in connection with the laying of the Atlantic cables called for his presence on board ship, and thus attracted his attention to the art of navigation, if indeed he could live in Glasgow without being in some measure drawn into it. He became a skillful yachtsman, and he used his yacht for testing improvements in the means of navigation. His achievements in this direction are numerous and important, but the principal ones are his improved mariner's compass, and his improved sounding line. The use of iron in the construction of ships introduces a serious interference with the compass needle; the needle may direct itself towards a point in the ship instead of a point in the Earth. The action of the ship's magnetism must be cancelled; and this is no easy matter in the case of the ordinary mariner's compass. The improved compass of Sir William Thomson had instead of one large needle, a number of very small needles placed parallel to one another; and instead of a heavy continuous card a light card with the centre wholly cut away. It is more steady, more free to move, and more easily protected from the ship's magnetism. His sounding-line consists of a sinker of 20 to 30 pounds, carried by a strong steel wire. The greatest vertical depth of the sinker beneath the surface is recorded by an instrument which measures the greatest water pressure; and it is read after the instrument has been brought back on board ship. In the old method of casting the lead the depth is determined from the length of rope run out. With the old method a ship must be brought to a standstill, if any trustworthy measure is desired in deep water; with the improved line, a steamer may be running at a speed of 20 knots.

Connected with navigation is his invention of a machine for calculating the heights of the tides at a given port. "It is essentially a mechanical contrivance by which the sum of

* One desiring to follow this celebrated controversy further should consult the article on Geology in the Eleventh Edition of the Encyclopædia Britannica.—EDITORS.

a Fourier series is obtained by mechanical means. The tides for a given part for a whole year can be wound out of it in four hours, thus facilitating their prediction to an extraordinary degree. The form in which it gives the prediction being a continuous curve on paper, it enables the height of the water at any moment to be ascertained by inspection, while any arithmetical result that could possibly be worth the trouble of calculating, would only give the times of high and low water."

Sir William Thomson was for many years a member of the Committee of the British Association, which had in charge the development of an absolute system of units. He was the champion of the centimeter as opposed to the metre; and his argument was that it was important that the density of water should be unity, not 1,000,000. Electrical measurement was then in its infancy. Looking at the question in the light of recent development, we see that the adoption of the centimeter was a mistake for the desired system of C.G.S. electric units is too small for practical purposes, and the actual system which is used involves the fundamental units multiplied by some power of ten. Hence electrical computations now include the metric system proper, the C.G.S. system, and the practical electric system. Sir William Thomson designed many instruments for the purpose of electrical measurements, and for the manufacture of these instruments established a large workshop in Glasgow, under the management of James White. This has been a principal source of his fortune.

When I was at work in Tait's laboratory, Sir William Thomson was president of the Royal Society of Edinburgh; and I have often heard him read papers and make addresses. These meetings brought him to Edinburgh frequently, and it was his custom to visit the laboratory of his colleague Tait. He must originally have been about six feet high. But for many years his height has been diminished by a stiff leg which was brought about in the following way. He broke his leg when skating on the ice, and would not remain at rest until it had recovered properly. Otherwise his appearance was athletic. Compared with Tait, he was not so elegant a speaker, but his

papers have more of the stamp of a genius. He has strong opinions on most subjects, and like most Irishmen, he is not afraid of a controversy. If Tait made a move and was not immediately successful, he was apt to retire resolved to have nothing further to do with it; not so Thomson; if baffled, he returns to the attack again and again On social matters he has strong conservative opinions; at a club meeting after the regular meeting of the Royal Society of Edinburgh he was asked: "Sir William! what do you think? Should a man be allowed to marry his widow's sister." "No sir, the Bible forbids it, and I hope the law of the land will continue to forbid it."

Sir William Thomson visited America at the time of the Centennial Exposition at Philadelphia, 1876, and he brought back to Scotland a wonderful account of Graham Bell's telephones. In 1884 he made another visit, to deliver a course of lectures at the Johns Hopkins University. This course of lectures, twenty in all, treated of the wave-theory of light, principally with the outstanding difficulties of the theory, and they partook largely of the nature of conferences. "Discussion did not end in the lecture-room; and the three weeks over which the lectures extended, were like one long conference." He was also a member of the Commission which solved the problem of harnessing Niagara.

In 1892 he was created a member of the House of Lords, under the title of Baron Kelvin. He took his title from the stream which flows past the hill on which the University of Glasgow is built. In 1896 the jubilee of his professorship was celebrated with great éclat at Glasgow. The exercises lasted three days and there were present representatives from all the scientific institutions of Great Britain, and from many of the scientific institutions of other countries. After a further tenure of three years, he resigned his chair. He now spends his time mostly at his country seat at Largs on the coast of Ayrshire, and at his house in London. The degrees and honors conferred upon him are numbered by hundreds, and the enumeration of these honors might be most briefly made by mentioning the

few not conferred; he is still open, I believe, to receive some distinguished mark of recognition from the geologists.

Lord Kelvin has been twice married, but there is no direct heir to inherit either his genius or title. Notwithstanding the fact that he has long been the acknowledged leader of science in Great Britain, and indeed in Europe, his disposition has remained simple and kindly. A multitude of honors, and great fame and power has not spoiled the grandson of the small Irish farmer. He is still active in the production of scientific papers, and although now nearly 78 years of age is making preparations to again cross that ocean which has been the scene of so many of his exploits, and which is now much more safely navigated through the instrumentality of his inventions.*

* Lord Kelvin died on December 17, 1907, in the 84th year of his age. His activity in scientific discussions did not diminish with age. He revised the lectures on the wave-theory of light which he had delivered at Johns Hopkins University and published them in 1904. In that year also he was elected Chancellor of the University of Glasgow. He continued to take an active part in the work of scientific societies; only a few months before his death he delivered at the meeting of the British Association a long and searching address on the electronic theory of matter. He was buried in Westminster Abbey a few feet south of the grave of Newton.—EDITORS.

## CHARLES BABBAGE *

(1791–1871)

Charles Babbage was born at Totnes in Devonshire on December 26, 1791. His father was a banker and was able to give his son a moderate fortune. Being a sickly child he received a somewhat desultory education at private schools, first at Alphington near Exeter, and later at Endfield near London. It appears that he instructed himself in the elements of Algebra, and that he early manifested a great fondness for it.

When he entered Trinity College, Cambridge, in 1810, he was already acquainted with the text books of Lacroix and other French writers; he had also read the book of Woolhouse which aimed at introducing into Cambridge the Leibnitzian notation for the differential calculus. Among his contemporary graduates he found congenial spirits in Peacock and Herschel, and the three friends, along with some juniors such as Whewell, were wont to breakfast together each Sunday morning and discuss philosophical subjects. At one of these philosophical breakfasts the "Analytical Society" was formed, the object of which as stated by Babbage was "to advocate the principles of pure *d*-ism in opposition to the *dot*-age of the University. Babbage was skillful in getting up what the politicians call a good cry. It was while he was yet an undergraduate that an idea occurred to him which ruled the whole of his subsequent career. One evening he was sitting in the rooms of the Analytical Society at Cambridge, his head leaning forward on the table in a dreamy mood, with a table of logarithms lying open before him. Another member, coming into the room and seeing him half asleep, called out "Well, Babbage, what are you dreaming

* This Lecture was delivered on April 21, 1903.—Editors.

about?" to which he replied "I am thinking that all these mathematical tables might be calculated by machinery."

In the last year of his undergraduate career, he migrated from Trinity College to Peterhouse, and did not compete for honors, believing Herschel sure of the first place, and not caring to come out second. He took merely a pass degree in 1815, and thereafter resided in London, where philosophical breakfasts continued to be a feature of his house. In the year following the text-book of Lacroix *Differential and Integral Calculus*, translated by Herschel, Peacock, and Babbage, was published by the Analytical Society; and four years later a volume of *Examples on the Calculus*. Lacroix had also written on the calculus of Finite Differences, and both Herschel and Babbage were attracted to the subject. The latter immediately contributed three papers on "The Calculus of Functions" to the Royal Society and he was elected a Fellow at the age of twenty-five.

He married, and made a tour of the Continent. He visited Paris and studied the details of the arrangement by which the celebrated French tables had been computed under the direction of Prony; and he copied the logarithms to fourteen places of figures of every 500th number from 10,000 to 100,000 from the manuscript tables deposited in the observatory at Paris. These tables were computed at the time of the Revolution, in order to facilitate the application of the decimal division of the degree which had been adopted. In executing the task Prony received a valuable hint from Smith's *Wealth of Nations* where the "division of labor" is exemplified. He adopted the idea; appointed three classes of mathematical workers; first, five or six analyists to investigate the best formulæ; second, seven or eight mathematicians to calculate arithmetical values at suitable intervals; and third, sixty or eighty arithmeticians (said to have been tailors on a strike) to compute intermediate values by the method of differences. The tables thus computed fill seventeen large folio volumes.

On his return to London he was encouraged by Wollaston (a pioneer in electrical science) to set about the realization

of his idea of a difference machine for computing tables. What is the fundamental idea of the method of differences? Write down the square numbers in the first column, the differences

| Squares | First Differences | Second Differences | Third Differences |
|---|---|---|---|
| 1 | 3 | 2 | 0 |
| 4 | 5 | 2 | 0 |
| 9 | 7 | 2 | 0 |
| 16 | 9 | 2 | 0 |
| 25 | 11 | 2 | |
| 36 | 13 | | |
| 49 | | | |

between the successive squares in the second column, and the differences of the first difference in the third column; these last are constant, consequently the next differences are all zero. To compute a table of squares, then, it is only necessary to add to a square the preceding first and second differences, thus $49+13+2=64$, etc. In the case of logarithms and other transcendental functions there is no difference which becomes zero, but when a certain number of figures only are required, there is a difference which is zero within a certain range. Hence within that range the same process of calculation may be applied as for a function which has a certain order of differences constant. To calculate tables by a machine only a device for adding is required; to insure accuracy in the printed tables Babbage thought it necessary that the machine which computes the results should also print them.

By 1822 Babbage had constructed a small model having two orders of differences and applicable to computing numbers of from six to eight places. It could compute squares, triangular numbers, values of $x^2+x+41$, and values of any function of which the second difference was constant and not greater than about 1000. He exhibited this model to the Royal Astronomical Society and was subsequently awarded a gold medal on account of it. He also wrote a public letter to Sir Humphrey Davy, then president of the Royal Society, explaining the utility of his invention. Through what had been published the

Government was induced to apply to the Royal Society for an opinion on the merits and utility of the invention; it appointed a committee which reported favorably. The Government advanced £1500 and work was started in 1823. Babbage superintended the work, and he employed a mechanical engineer, named Clement, whose workshop was in Lambeth, to execute his plans. The construction of a Difference Engine was begun having six orders of differences, each consisting of about twenty places of figures, and provided with mechanism to print the results. It was called an Engine, because after being started with the proper differences for computing a table the results would be produced merely by power applied to a shaft.

Three years later (1826) the Lucasian professorship of mathematics at Cambridge became vacant. There were three candidates; French, who was the head of one of the colleges; Airy, afterwards astronomer royal; and Babbage. The appointment is made by the heads of the colleges, and in this case they were quite prepared to appoint a candidate from their own number who was more proficient in divinity and Hebrew than in mathematics. This was Newton's chair, but since his time mathematics had declined at Cambridge and was only now beginning to revive. Babbage threatened legal proceedings, with the result French retired and Airy was elected. Airy resided and lectured, the first Lucasian professor who had done so for many years; two years later he changed to the professorship of Astronomy, and his former rival Babbage was elected. This was in 1828; although Babbage held this professorship until 1839 he did not reside or lecture; his mind was completely absorbed with anxiety about the success and fame of his computing machine. However, with a view of delivering a course of lectures, he collected the material and published a book called *Economy of Machinery and Manufacture* which he dedicated to the University of Cambridge. The object of the volume is to point out the effects and the advantages which arise from the use of machines; to endeavor to classify their modes of action and to trace the consequences of applying machinery to supersede the skill and power of the human arm. Babbage wrote

many books, but this is considered his most finished production; it has been described as a "hymn in honor of machinery."

The work on the Difference Engine went on for five years with little interruption, and the expenses had amounted to nearly £7000, of which the Government had advanced less than half, the remainder having come out of Babbage's pocket. Before proceeding further he wished to have a complete understanding with the Government, which was eventually reached after a delay of two years. The Government repaid Babbage what he had advanced, arranged to pay certified bills, leased a part of the grounds belonging to Babbage's house, and erected thereon a fireproof office and workshops. While these were in course of erection, the work continued for three years longer in Clement's workshop. At the end of this time (1833) a portion of the Difference Engine was assembled, and found to fulfill all Babbage's expectations and even more.

The Royal Society, like the University of Cambridge, had also declined as a scientific center since the days of Newton. The president had often been one of high rank rather than eminent in science. At this time the reforming party put up Sir John Herschel as a candidate for the presidency in opposition to the Duke of Sussex, but the royal candidate was successful. Babbage was one of the leading reformers; he prepared and printed a book called *The Decline of Science in England* which proved highly beneficial in that it led in a short time to the foundation of the British Association for the Advancement of Science.

After the drawings and parts of the computing machine were removed to the fireproof premises adjoining Babbage's house, the engineer Clement made a claim for compensation for the removal of his business from Lambeth, a claim which Babbage declined to entertain as being extravagant. Whereupon Clement stopped the work on the machine, disbanded the specially trained workmen, and carried off all the tools, including those specially designed by Babbage and paid for by the Government. This he could do according to English law; he offered to sell the special tools to Babbage but the latter

declined purchasing. Notwithstanding this bad break, the Government were willing to proceed; and the construction was actually in an advanced state. Among the workmen discharged by Clement was Joseph (afterwards Sir Joseph) Whitworth who later amassed a fortune by utilizing as a mechanical engineer the training which he got from Babbage.

While the work was suspended owing to change of workshop, Babbage experimented much with the portion of the engine which had been assembled; and his inventive mind conceived the idea of a much more general machine which he called an Analytical Engine. He immediately set to work to plan how it could be realized, and he considered that he had hit upon a much simpler mechanical invention for adding than the one adopted in the Difference Engine. Unfortunately instead of proceeding to complete the Difference Engine as the plans adopted and followed for ten years, as the Government desired him to do, he waited for an opportunity to explain about his new invention. However superior his new ideas might be, he ought to have perceived that the heads of the Government—and that Government frequently changing—were not capable of appreciating their value, and that they would judge of the matter from the business point of view, saying "You wish us to abandon the construction of an engine which has cost £17,000 and wish us to undertake a new and more elaborate engine; we cannot justify such expenditure to the House of Commons."

It was very unfortunate that Babbage did not see the practical necessity of completing the first engine on the plans adopted. By the course he adopted he gave his scientific enemies a chance to defeat the realization of his great invention. The matter was not finally settled until 1842—nine years after the construction was suspended. He was then informed by the Premier and the Chancellor of the Exchequer that they abandoned the undertaking, and that he might have what had been constructed for his own property. Babbage declined to accept it; the portion assembled was placed in a museum; the loose parts sold or melted down. Babbage appears to have thought that

the Ministers acted on their own judgment, but it was not so; Airy, the astronomer at Greenwich, records in his *Autobiography* that he was consulted and that he pronounced the Difference Engine to be worthless. Naturally the ministry attached great weight to this opinion, for the immediate value of this engine was claimed to be the construction of astronomical and nautical tables.

The portion of the Difference Engine which was put together has been exhibited at various Expositions in London, and is now in the Science and Art Museum at South Kensington; I saw it, and heard it explained, on the occasion of the Loan Exhibition of Scientific Apparatus in 1876. It consists of three columns; each column contained six cages, each cage one figure wheel. Each figure wheel has the numbers 0 to 9 placed around the circumference and may be set by hand at any one of the numbers. The right-hand column is for the resulting number, the middle column for the first difference, and the left-hand column for the second difference. Suppose any sets of proper numbers to be placed upon the three columns, then the mechanism is such that four half turns of the handle—two backwards and two forwards—causes the first difference to be added to the previous result and the second difference to be added to the first difference; hence if the machine printed the results, mere turning of the handle would produce the entire table of numbers or all the results requiring to be interpolated between two given values. To make the portion assembled more useful, slight departures from the general plan were adopted. The three upper wheels of the left-hand column were separated from the rest of the machine and employed to count the natural numbers, that is, to register the number of calculations made and give the numbers corresponding with the terms of the table computed. A wheel at the top of the central column indicated when each calculation is complete and also the position of the handle when the figure wheel was to be adjusted.

About this time (1829) the Earl of Bridgewater died, leaving a sum of £10,000 to trustees to be expended in the production of books "on the Power, Wisdom, and Goodness of God as

manifested in the Creation," the writers to be selected by the President of the Royal Society. He, acting with certain bishops, selected eight authors, assigned to each a portion of the subject with an honorarium of £1250. Babbage was not one of the number, but in 1837, after the eight treatises had appeared, he published a volume entitled *The Ninth Bridgewater Treatise.* In design the book is grand and much superior to the regular treatises, but in execution it is like many others of Babbage's works, a magnificent torso. He was moved to write the book by a chapter in Whewell's Bridgewater volume where it is maintained that long application to mathematical and physical reasoning disqualifies the mind from duly appreciating the force of that kind of reasoning which alone can be adduced in favor of Natural Theology. Babbage thought that such reasoning tended to promote the prejudice that the pursuits of exact science are unfavorable to religion; he shows on the contrary that his pursuits had led him to new views of the truths of Natural Theology.

The most remarkable part of Babbage's book is where he takes up Hume's conception of a law of nature, and the consequences as to miracles which he deduced from it. According to Hume cause and effect are nothing more than invariable sequence; and a law of nature rests upon experience or repeated observation just as the reliability of a witness does. Babbage points to his Difference Engine (that is, the part completed) and remarks that it may be adjusted to produce the natural numbers. He asks a supposed observer how often a natural number must be produced to infer that this is the whole law of the machine; one hundred times? one thousand times? one million times? Babbage answers that according to the constitution and given adjustment of the machine it will produce the natural number up to 1,000,001; but after that it will give the triangular numbers and that after 2761 turns a further complexity will be introduced. These additional complexities are necessary consequences of the nature and given adjustment of the machine; and no amount of mere induction from given instances could detect the inner necessary connection. Hence ~~casual~~ causal connection

and repeated sequence are not the same thing. He went on to prove by his Analytical Engine (existing only in drawings) that "It is more probable that any law, at the knowledge of which we have arrived by observation, shall be subject to one of those violations, which, according to Hume's definition, constitutes a miracle, than that it should not be so subjected." He rests this proposition on the statement that his Analytical Engine could be set to compute the successive terms of a given algebraic law, but so that one chosen term would be different, and then to resume the production of the true terms ever after. Provision could be made by the maker of the machine for a single suspension of the law at a given point.

Babbage devoted 37 years of his life to perfecting the invention of the Analytical Engine and no inconsiderable part of his fortune was spent thereon. This invention must be carefully distinguished from the Difference Engine; they are often popularly confounded but are confused in some scientific writings. When the fragment of the Difference Engine was put together in 1833, Babbage found that, as he had anticipated, it possessed powers beyond those for which it was intended, something in the same way as algebra displays powers beyond those of generalized arithmetic for which it was designed. Babbage saw that, by interposing a few connecting wheels, the column of Result could be made to influence the last Difference, and he proposed to arrange the axes circularly so that these columns should be near each other. He called this arrangement "the engine eating its own tail." This soon led to the idea of controlling the engine by entirely independent means, and to the idea of an engine which could calculate the numerical values of any function which the mathematician can express in a series of integral powers.

To realize the first idea—that is, to make the adjustment of the engine automatic—he had recourse to the device of punched cards similar to those invented by Jacquard for the weaving loom. The machine was to consist of three parts; first, the store; second, the mill; third, the cards. The store was to consist of 100 columns each of fifty wheels for indicating

the given numbers, intermediate numbers, and resulting number. The mill was to consist of mechanism which would add two numbers, subtract a less number from a greater, multiply two numbers, or divide one number by another, according to the kind of gearing brought into operation. The cards were of three kinds; Number cards to communicate given numbers to the store; Directive cards to transfer numbers from the store to the mill and from the mill to the store; Operation cards to call for addition, subtraction, multiplication, division. For example, to compute numerical values of $(ab+c)d$, seventeen cards in all were required, as follows:

| Number Card | Directive Card | Operation Card | |
|---|---|---|---|
| 1 | | | Places $a$ on column 1 of store. |
| 2 | | | Places $b$ on column 2 of store. |
| 3 | | | Places $c$ on column 3 of store. |
| 4 | | | Places $d$ on column 4 of store. |
| | 1 | | Brings $a$ from store to mill. |
| | 2 | | Brings $b$ from store to mill. |
| | | 1 | Multiplies $a$ and $b=p$. |
| | 3 | | Takes $p$ to column 5 of store. |
| | 4 | | Brings $p$ into mill. |
| | 5 | | Brings $c$ into mill. |
| | | 2 | Adds $p$ and $c=q$. |
| | 6 | | Takes $q$ to column 6 of store. |
| | 7 | | Brings $d$ into mill. |
| | 8 | | Brings $q$ into mill. |
| | | 3 | Multiplies $d$ and $q=r$. |
| | 9 | | Takes $r$ to column 7 of store. |
| | 10 | | Takes $r$ to printing apparatus. |

Each form of calculation would require a special set of cards strung together in proper order; just as the particular pattern for a woven fabric requires its own set of Jacquard cards, and they would be applied to the calculating machine in the same manner. The great improvement in the construction of the engine proper was the invention of the principle of the Chain, by which the carriage of the tens is anticipated. This part of the design was actually constructed. For subtraction the adding rotations were reversed; multiplication was to be effected by successive additions, and division by successive subtractions. It is obvious that the machine could treat of

transcendental functions only when expressed in a series of powers. Irrational quantities would be represented approximately.

To express the complicated relations among the various parts of the machine, Babbage invented what he called a "mechanical notation" explained in a paper published in the *Philosophical Transactions* for 1826, entitled "On a method of expressing by signs the action of machinery." It consists of three divisions; first, Notation for the parts; second, Representation of trains; third, Representation of cycles. He denoted pieces and points of the frame by upright letters, the former capitals and the latter small letters; movable pieces and their points by slant letters, capitals and small letters respectively. On account of the great number of movable pieces he employed indices, placing them to the left above the letters. The train is designed to show how motion is transmitted from the prime motor to the final driven piece. The several pieces are marked on a diagram by trial so that each pair of driver (point) and driven (piece) may be connected by arrows; after a number of trials the pieces are so placed as to make the connecting arrows the shortest. In a cycle he aimed at representing the time during which each piece moved and the time of action of each of its working points. The period of the machine is represented by a vertical line divided into proper subdivisions on the nature of the machine; to each piece and to each working point is allotted a parallel line, and those portions of the period are marked off during which there is no movement of the piece or the point, thus giving a synoptic view of the motion of the machine. To make drawings, perfect the notations, and test mechanical contrivances, he turned his coach house into a forge and foundry, transformed his stables into a workshop, and expended a large sum in employing skilled workmen.

In 1840 he received a letter from M. Plana, nephew of Lagrange, urging him to come to a meeting of Italian philosophers which was to be held in Turin. Babbage went, furnished with models, drawings, and notations of his Analytical Engine, and explained them to the Italian mathematicians, among whom was M. Menabréa. Subsequently Menabréa wrote an

account of the invention in French, which was afterwards translated into English and embellished with notes by Lady Lovelace, née Augusta Ada Byron, daughter of the poet Byron. This lady did not inherit the poetic genius of her father, but was remarkable for exact mathematical attainments, which were also possessed by her mother.

Babbage himself never wrote an extended account of the Analytical Engine; the memoir of Menabréa with the notes of Lady Lovelace gives the most complete account regarding it. In 1848 he made drawings for a new Difference Engine in which the adding was to be effected by his new contrivance. He was anxious to discharge whatever imagined obligation might be supposed to rest upon him in connection with the original undertaking, and an entirely practicable proposal was laid before the Premier (Lord Derby) by Lord Rosse, a mathematical nobleman. The Premier turned the matter over to his Chancellor of the Exchequer, Benjamin Disraeli, who gave an adverse decision. The wrath of Babbage at the novelist was unbounded; he denounced him as the Herostratus of Science. A few years later a Difference Engine, suggested by Babbage's plans, was actually constructed in Sweden by a printer named Scheutz; it performed successfully the kind of work for which it was designed. The original Scheutz machine was bought by the Dudley Observatory at Albany, N. Y., and was used to a slight extent about 1878; a copy of it constructed for the English Government has been used for the calculation of insurance tables.

After the death of Babbage in 1871 what he had accomplished on the Analytical Engine was transferred for safe-keeping to the Museum at South Kensington. The British Association appointed a committee to examine it; in 1878 they reported that the part assembled was only a small portion of the mill sufficient to show the methods of addition and subtraction; that the drawings were complete in exhibiting every movement essential to the design of the machine. They concluded that the labors of Babbage, first on the Difference Engine, and afterwards on the Analytical Engine were a marvel of mechanical

ingenuity; that the realization of the latter would be of utility; that the complete design is not more than a theoretic possibility; and that the mill portion of it might be constructed at reasonable expense.

Babbage was distinguished for his skill in solving ciphers. He wrote a paper "On the properties of letters occurring in various languages" and it appears that these researches gave the keys which he used. In 1851 he communicated to the Trinity House a note respecting occulting lights in lighthouses. His idea of making each lighthouse publish its own name was forthwith adopted by the English and American Governments. The application of the same idea to solar light led to the invention of the heliograph, first brought into practice by the Russians at Sebastopool and which figured so prominently in the siege of Ladysmith.

Babbage's last book, published in 1864, was a kind of autobiography entitled *Passages from the Life of a Philosopher*. Like many of his works it was brilliant in conception but incomplete in execution. In his later years he came before the public as the implacable foe of organ grinders. He estimated that one-fourth of his entire working hours had been wasted through audible nuisances to which his highly strung nerves rendered him peculiarly sensitive.

Charles Babbage died on October 18, 1871 in the 80th year of his age. To the public he was known as an eccentric and irritable person, as a crank on the subject of calculating machines. But his books show true nobility of nature; his engines exhibit marvelous mechanical ingenuity. He sowed many valuable seeds which less able but more thrifty minds turned to advantage. As a reformer he accomplished much for exact science, especially in the foundation of the Astronomical Society, the British Association, and the Statistical Society. The money expended by the Government on his machine was fully repaid, according to Lord Rosse, by the improvement in mechanical tools which he made incidentally in his designs. The main defect in his character was a want of persistence and an imperfect adjustment of his aims to what was practicable.

## WILLIAM WHEWELL *

(1794–1866)

WILLIAM WHEWELL was born at Lancaster, England, on May 24, 1794. His father was a master carpenter and had several children. William was educated first at the grammar school of his native town, and was afterwards sent to that at Heversham in order to qualify for an Exhibition to Trinity College, Cambridge. The winning of this exhibition of £50 was his first scholastic success. At these schools great attention was paid to classical studies, including versification in both Latin and Greek, and he also received a good start in mathematics. He entered Trinity College in October, 1812. The Analytical Society was then in existence and he became one of the group which met on Sunday mornings to breakfast and to discourse on philosophical subjects. One of the principal honors which he gained in his undergraduate career was the Chancellor's medal for the best poem on Boadicea, in the course of which he celebrates the praises of beauty:

> O beauty! heaven born queen! thy snowy hands
> Hold the round earth in viewless magic bands;
> From burning climes where riper graces flame
> To shores where cliffs of ice resound thy name,
> From savage times ere social life began
> To fairer days of polished, softened man;
> To thee, from age to age, from pole to pole,
> All pay the unclaimed homage of the soul.

Whewell did not concentrate his attention exclusively on the subjects of the final examination, but he came out second wrangler. The next year (1817) he won a fellowship, took

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private pupils, and began to read extensively with a view of following in the footsteps of Francis Bacon. He was soon appointed one of the mathematical tutors of his college. His connection with the Analytical Society suggested his first work *An Elementary Treatise on Mechanics*, in which the continental notations for the calculus is used. It was a great advance on any existing text-book on the subject used at Cambridge; subsequently it passed through several editions and became much altered. In 1820 Whewell was one of the moderators at the tripos examinations, and, following the example of Peacock the year before, he made use of the *d* notation. He was not an ardent reformer like Peacock; he appeared to have waited until the success of the movement was apparent. Although his first book was on Mechanics, his main design, even then, was a work on the inductive philosophy in which he should take full advantage of what had been accomplished in the physical sciences since the time of Bacon. For this reason we find him at an early age studying Locke's *Essay on Human Understanding* and Kant's *Critique of Pure Reason*.

In those days a fellowship expired at the end of seven years unless the holder took holy orders. Whewell took orders in 1826; in the same year he and Airy, the Lucasian professor of mathematics, made observations in a mine in Cornwall to determine the mean density of the earth. Bacon, more than two centuries earlier, had suggested swinging a pendulum in a deep mine for this purpose. Airy and Whewell attempted to determine the time of oscillation of a pendulum at the bottom of the mine—about 1200 feet deep—and to compare it with that of another pendulum on the surface. An accident to the pendulum vitiated the first series of observations. Two years later they made a second series, which was also unsuccessful on account of an accident in the mine. Nearly thirty years later Airy, however, made successful observations at another mine from which he deduced 6.565 as the mean density of the earth as compared with water. At the time when Whewell took orders the professorship of mineralogy at Cambridge fell vacant; it appears to have been occupied as a sinecure. Whewell

saw in it a position where he might have opportunity to study one of the sciences comprehended in his scheme of inductive philosophy. He held the appointment for several years, delivered lectures, founded a museum and wrote an essay on mineralogical classification. In 1830 he published a book on the architecture of Gothic churches, in which he gave a theory explaining how the Gothic style had been derived from Grecian and Roman architecture.

It suited the philosophic plans of the professor of mineralogy to study the new and allied science of geology. In 1830 the first volume of Lyell's *Principles of Geology* appeared in which was adopted and extended the doctrine of uniformity first published by Hutton. Whewell believed in the older doctrine of successive catastrophes; in a review of the book he said: "Hutton, for the purpose of getting his continents above water, or of manufacturing a chain of Alps and Andes, did not disdain to call in something more than the common volcanic eruptions which he read of in newspapers from time to time. He was content to have a period of paroxysmal action, an epoch of gradual distraction and violence, to usher in one of restoration and life. Mr Lyell throws away all such crutches; he walks alone in the path of his speculations; he requires no paroxysms, no extraordinary periods; he is content to take burning mountains as he finds them; and with the assistance of the stock of volcanoes and earthquakes now on hand, he undertakes to transform the earth from any one of its geological conditions to any others. He requires time, no doubt; he must not be hurried in his proceedings. But if we will allow him a free stage in the wide circuit of eternity, he will then ask no other favor." Whewell here seems to adopt that view of geological time which has since been advocated by Kelvin. This same year there appeared Herschel's work *Preliminary Discourse on the Study of Natural Philosophy*. Herschel's object was to extend and correct the inductive philosophy of Bacon in the light of later achievements. Whewell was, from his own plans deeply interested in this work and he wrote a review in which he remarked that Herschel had said nothing of Bacon's condem-

nation of the method of *anticipation* of nature, as opposed to what he considers as the true method of *interpretation* of nature. As a matter of fact Herschel was too wise to follow Bacon in his condemnation of anticipation; he knew that the guidance of theory was needed for the interpretation of facts.

Whewell was one of the eight persons selected to write the *Bridgewater Treatises.* His subject was "Astronomy and General Physics considered with reference to Natural Theology," and he received £1000 as well as the profits of the volume. His treatise is divided into three parts: (1) Terrestrial adaptations, (2) Cosmical, arrangements, (3) Religious views. In the first part he aims at demonstrating how the laws and facts of nature work in harmony to secure the well being of man, animals, and plants; and the inference is drawn that such arrangement testifies to the existence of an intelligent and beneficent Creator. In the second part he shows how all the universe is subject to a law of continual decay. The third part has two remarkable chapters on inductive and on deductive habits, the former, he held, had a stronger tendency to religion. This volume was the most popular of the eight *Bridgewater Treatises,* and it went through seven editions.

Soon after the British Association was founded in 1831 Whewell became one of the most active members; to him is due the important suggestion of the preparation, by committees or specially appointed individuals, of reports upon subjects of scientific importance and their publication in full in the Proceedings. In 1833 he was one of the secretaries of the meeting of the Association held at Cambridge and it fell to him to deliver an address similar to the presidential addresses of later years. By this time Whewell had acquired the reputation through his philosophical researches of being the best authority in Great Britain on scientific language. Faraday in his electrolytic researches had encountered a number of new ideas and for these he wished to have suitable names. Whewell suggested *anode, cathode, anion, cation, ion,* words which, with their derivatives, are now familiar not only to the electrician, but to people of general culture. Other electrical terms sug-

gested by Whewell to Faraday were *paramagnetic* and *diamagnetic*. To Lyell, the geologist, he suggested *eocene*, *miocene*, *pliocene*.

In 1833 Whewell published in the *Transactions* of the Royal Society the first of a series of memoirs on the tides. In its preface he says: "No one appears to have attempted to trace the nature of the connection among the tides for the different parts of the world. We are, perhaps, not even yet able to answer decisively the inquiry which Bacon suggested to the philosophers of his time, whether the high water extends across the Atlantic so as to affect contemporaneously the shores of America and Africa, or whether it is high on one side of the ocean when it is low on the other, at any rate such observations have not yet been extended and generalized." To this subject Whewell applied his method of the colligation of facts, more commonly called the reduction of observations. His main object was to reduce the enormous series of observations concerning the tides which had accumulated, and in this work he had the aid of skilled computors paid by the Admiralty or by the British Association. He began by constructing a map of *cotidal lines* for the whole globe, that is, lines drawn on the surface of the ocean and passing through all the points where it is high water at the same time. The succeeding memoirs were devoted to the discussion of observations at London, Liverpool, Plymouth, and other ports. There were fourteen of these memoirs and Airy thus estimates their value: "Viewing the two independent methods introduced by Mr. Whewell, of reducing the tabular numbers to law by a process of numerical calculation, and of exhibiting the law to the eye without any mathematical operation by the use of curves, we must characterize them as the best specimens of reduction that we have ever seen." Whewell did not grapple with the theory of the tides, that he left—to use his own words—"to bolder and stronger mathematicians." Neglect of the rôle played by theory, especially mathematical theory, in the discovery of truth, is the weak point in Whewell's philosophy. The reduction of observations to empirical laws is only one step in the process and not the most important.

In 1835 Whewell published a pamphlet on mathematics in liberal education—one of the fruits of his philosophical studies. In it he maintains that mathematics is superior to formal logic as an educational discipline, and he discusses faults in teaching by which its benefits are diminished. In reply to the pamphlet an article appeared in the *Edinburgh Review*, written by Sir William Hamilton, professor of logic and mathematics at Edinburgh, which became notorious as a wild and indiscriminate attack on mathematical work by a person only slightly acquainted with it. In the succeeding number Whewell asks for the titles of some treatises on practical logic and philosophy which the reviewer would recommend for their educational efficiency as rivals to the well-known mathematical treatises. In this tilt between the expounder of the renovated Baconian logic and an official representative of the old scholastic logic, the modern champion came off victorious.

In 1837 Whewell finished the first part of his *History of the Inductive Sciences*. In this book he notes the *epochs* when the great steps were made in the principal sciences, the *preludes* and the *sequels* of these epochs, and the way in which each step was essential to the next. He attempts to show that in all great inductive steps the type of the process has been the same. The prominent facts of each science are well selected and the whole is written with a vigor of language and a facility of illustration rare in the treatment of scientific subjects. This book was, however, introductory to his *Philosophy of the Inductive Sciences* which appeared three years later; its preparation had indeed gone along with that of the *History*. In this work Whewell explained the process of induction, the elements of which it consists, what conditions it requires, and what facilities it calls into play. He maintains that, in order to arrive at knowledge or science, we must have besides impressions of sense, certain mental bonds of connection, ideal relations, or ideas. Thus *space* is the ideal relation on which the science of geometry depends; *time*, *cause*, *likeness*, *substance*, *life*, are ideal relations on which other sciences depend. Whewell's philosophy was, in fact, a blending of Kant and Bacon.

Bacon recommended that a great collection of facts should be made regarding every branch of human knowledge, and conceived that, when this had been done by common observers, philosophers might extract scientific truth from these facts by the application of a right method. As an example of such an investigation Bacon collected facts bearing on the nature of heat and he arrived at the conclusion " That heat is an expansive, restrained motion, modified in certain ways, and exerted in the smaller particles of the body." This true conclusion was designated by Bacon as a " first vintage," in other words as a guess, but it was regarded by Whewell as an unfortunate conclusion, and he asks " Where is the motion in a red-hot iron?" Whewell made a great advance on the method of Bacon by claiming that ideas are as indispensable as the facts themselves, and that facts are collected in vain except they be duly unfolded by ideas; his defect was that he stopped short at ideas, instead of proceeding to theories and equations.

In 1841 he was president of the British Association for the meeting at Plymouth. His address was characteristic; he compared the Association to Solomon's House, imagined by Bacon in *The New Atlantis*, the principal difference being that the Association depended upon voluntary support, whereas the philosophers of Solomon's House were to be paid by the state. This House had caves and wells, chambers and towers, baths and gardens, parks and pools, dispensatories and furnaces, and other provisions for experiment and observation. " There were also many classes of persons to conduct the business of the college: merchants of light, mystery men, depredators, pioneers or miners, dowry men or benefactors, inoculators, and finally *interpreters of nature* who elevate the truths of experiment into general laws which are the highest form of human knowledge." The imaginary teacher who thus described Solomon's House to a traveler also said: " The end of our foundation is the knowledge of causes and secret motions of things." But Whewell said: " Knowledge is to be dealt with as the power of interpreting nature and using her forces,

not as a power of exciting the feelings of mankind and providing remedies for social evils."

In the interval between the publication of the *History* and the *Philosophy* Whewell took a step which may appear erratic, but which in reality was a step toward the accomplishment of his great plan. He accepted the Chair of Moral Philosophy. In a letter he explained that this was done so that he might ultimately extend his inductive principles to some of the metaphysical sciences. He proposed to resign his position as a mathematical tutor and to take a college living in the country. In 1841 he was 47 years old and engaged to be married. But finally, instead of retiring to the country, he bought a house in Cambridge. Shortly after he was married, and within a week he was appointed Master of his college—the foremost scientific college in England. He never occupied the house which he had bought; henceforth his home was Trinity College.

While Master of Trinity he published anonymously the book *Plurality of Worlds*, to which I referred in the lecture on H. J. S. Smith.* Fontenall and Chalmers had maintained the affirmative—that there is a plurality of worlds. Whewell maintained the negative and his book went rapidly through five editions. Brewster in *More Worlds than One* then took the affirmative side, this title being said to give " the Creed of the Philosopher and the Hope of the Christian." In more recent times Proctor wrote *Other Worlds than Ours* setting forth the results of scientific researches. Only a few months ago Whewell's old position was maintained in the *Fortnightly Review* by Mr. Wallace, but in a matter of astronomical reasoning Proctor is a much safer guide than Wallace. Whewell was Master of Trinity College for 25 years; much of his time was taken up by the duties of administration, especially on account of the reform of the college which the Government carried out. His writing during this period was mainly on moral science, but he also brought out the second and third editions of his *Philosophy of the Inductive Sciences*. One of his acts was to present a statue of Bacon to Trinity College.

* Ten British Mathematicians, p. 92.—EDITORS.

Whewell was noted for his power as a University preacher. He was a man of splendid physical development. A Cambridge legend tells of a prize-fighter who had exclaimed "What a man was lost when they made you a parson!" No doubt his friends imagined him hale and hearty at a very advanced age; but it was not to be. He was fond of horseback exercise and it was this recreation which cut short his career. His horse bolted and threw him, and the injuries were such that he died in a few days. His death occurred on March 6, 1866, in the 72d year of his age. He was twice married, but, having no children, bequeathed the most of his fortune to Trinity College.

He was very fond of argument and in early life, at least, somewhat rough in manner. De Morgan wrote: "The Master of Trinity was conspicuous as a rough customer, an intellectual bully, an overbearing disputant. The character was as well established as that of Sam Johnson, but there was a marked difference. It was said of Johnson that if his pistol missed fire he would knock you down with the butt end of it; but Whewell, in like case, always acknowledged the miss, and loaded again or not as the case might be. . . . I knew him from the time when he was my teacher at Cambridge, more than forty years ago. As a teacher he was anything but dictatorial, and he was perfectly accessible to the proposal of objections. He came into contact with me in his slashing way twice in our joint lives, and on both occasions he acknowledged himself overcome by that change of manner and apologetic mode of continuance which I had seen him employ toward others under like conditions." The great variety of his studies struck some of his contemporaries as peculiar; for instance Sydney Smith said at a breakfast party with reference to Whewell: "That man's forte is science and foible omniscience." There was, however, as we have seen, a method in his madness. In his day he was a Grand Master; in more recent times some have asked what contributions did he make to science. His enduring monument is the Renovation of the Baconian philosophy.

Whewell, like Bacon, set forth a series of aphorisms giving

the essentials of his philosophy. I will quote four of these: "I. Man is the interpreter of nature, Science the right interpretation. . . . VIII. The Sensations are the objective, the Ideas are the subjective part of every act of perception or knowledge. XI. Observed facts are connected so as to produce new truths by superinducing upon them an Idea; and such truths are obtained by Induction. XII. Truths once obtained by legitimate Induction are Facts; these facts may be again connected so as to produce higher truths; and thus we advance to successive Generalizations." On the title page of his later books you may find a picture of a hand transmitting a torch to another hand, with a motto of four Greek words underneath. The words are from Plato, who in allusion to an Athenian ceremony says: "Holding torches they will pass them on one to another." Whewell adopted the picture for his coat of arms with the motto *lampada tradam.*

## SIR GEORGE GABRIEL STOKES *

(1819–1903)

George Gabriel Stokes was born August 13, 1819, at Skreen, County Sligo, Ireland. His father was the rector of the parish, a clergyman of the Church of England in Ireland. When twelve years of age he was sent to a school in Dublin and two years later to Bristol College in the West of England. At this time the principal of that college was Dr. Jerrard whose researches on the solution of equations of the fifth degree were discussed by Sir William Rowan Hamilton at the Bristol meeting of the British Association in 1836. In 1837 young Stokes entered Pembroke College, Cambridge, and four years later graduated as senior wrangler, won the first Smith's prize, and was elected to a fellowship. During all his school and college years he had won distinction in mathematical studies.

He now did what was a great novelty in those days—turned one of his rooms into a physical laboratory. The University had no lecture rooms for its professors, far less laboratories or museums. Being a powerful analyst as well as a skillful experimenter he immediately entered on a period of fruitful scientific production. He chose as channels of publication the two institutions which had been recently inaugurated at Cambridge, namely, the Cambridge Philosophical Society, and the Cambridge and Dublin Mathematical Journal. To the former he contributed two papers on pure mathematical analysis, namely, " on the critical values of the sums of periodic series," based on Fourier's analysis of periodic functions; and another " On the numerical calculation of a class of definite integrals and infinite series," in which he was able to calculate the first

* This Lecture was delivered on April 28, 1903.—Editors.

fifty roots of an equation of which Airy had been able to calculate only two. Other memoirs followed: "On the theories of the internal friction of fluids in motion and of the equilibrium and motion of elastic solids," in which he shows for the first time how to take account of the equations of motion, of differences of pressure in different directions due to the viscosity of the fluid; and the resulting equations constitute the complete foundation of the hydrokinetics of the present day. "On the theory of oscillatory waves," in which he investigates the steep waves of the deep sea where the elevations are narrower than the hollows and the height of an elevation exceeds the depth of a hollow. "On the formation of the central spot of Newton's rings beyond the critical angle," his earliest investigation in the wave-theory of light. "On the dynamical theory of diffraction," containing the mathematical theory of the propagation of motion in a homogeneous elastic solid; also an experimental investigation from which he concluded that the plane of polarization is the plane perpendicular to the direction of vibration in plane-polarized light, agreeing with Fresnel's position as opposed to that of MacCullagh.

To the *Cambridge* and *Dublin Mathematical Journal* he contributed the following papers: "On the motion of a piston and of the air in a cylinder"; "On a formula for determining the optical constants of doubly refracting crystals"; "On attractions and on Clairault's theorem." A series of notes on hydrodynamics was prepared supplementary to a report on that subject which he presented to the British Association in 1846. Shorter papers he communicated to the Philosophical Magazine, two of which are the aberration of light and the constitution of the luminiferous ether viewed with reference to that phenomenon. On the theory of the emission of light, the explanation of aberration is simple; in these papers he attempts an explanation which shall be in accordance with the undulatory theory without making the startling supposition that the earth in its motion round the sun experiences no resistance from the ether.

In 1849 the Lucasian Chair of Mathematics at Cambridge fell vacant—the chair filled by Sir Isaac Newton 180 years

earlier. From 1828 to 1839 this chair was occupied by Charles Babbage who neither lectured nor resided; his successor, Joseph King, seems also to have made it a sinecure. But now the electors—who are the heads of the colleges—saw in Stokes a young, talented, and enthusiastic investigator who might worthily follow in the steps of Newton. At the time of the election Peter Guthrie Tait was an undergraduate and twenty-five years later he recorded his impression of the event: "To us, who were mere undergraduates when he was elected to the Lucasian professorship, but who had with mysterious awe speculated on the relative merits of the men of European fame whom we expected to find competing for so high an honor, the election of a young, and to us unknown, candidate was a very striking phenomenon. But we were still more startled, a few months afterwards, when the new professor gave public notice that he considered it part of the duties of his office to assist any member of the University in difficulties he might encounter in his mathematical studies. Here was, we thought (in the language which Scott puts into the mouth of Richard Coeur de Lion) "a single knight fighting against the whole mêlée of the tournament." But we soon discovered our mistake, and felt that the undertaking was the effort of an earnest sense of duty or the conscience of a singularly modest but exceptionally able and learned man. And as our own knowledge gradually increased and we became able to understand his numerous original investigations, we saw more and more clearly that the electors had indeed consulted the best interests of the University, and that the proffer of assistance was something whose benefits were as certain to be tangible and real as any that mere human power and knowledge could guarantee."

Tait himself benefited by this proffer of assistance; so did Thomson and Clerk Maxwell. In fact Prof. Stokes is regarded as the principal founder of the Cambridge school of mathematical physicists, one of the main glories of the British mathematicians of the nineteenth century, the only other name having any claim to the position being that of William Hopkins who tutored them all. Thus at the age of 35 years Stokes was placed

in the position where he was to do his life work. At that time the salary attached to the chair was small; the colleges collected all the revenues, and the University proper had very little for the payment of her officers.

Before his appointment to the Lucasian chair, Stokes had contributed a paper to the *Transactions* of the Royal Society of London: "On the theory of certain bands in the spectrum." He was now (1851) elected a Fellow of the Society. Two years later he was appointed one of the secretaries, an office which he continued to hold for thirty years. In 1852 he contributed an important paper "On the change of refrangibility of light" for which he received a Rumford medal, and which is considered his greatest contribution to science. Sir John Herschel had discovered a phenomenon, now called fluorescence, in the behavior of a solution of sulphate of quinine when a beam of light strikes on it. Viewed by transmitted light, the liquid appears colorless and transparent like water, but viewed by reflected light it exhibits a peculiar blue color. This blue color comes from a narrow stratum of the liquid adjacent to the surface by which the light enters. Light, which has once produced this effect, though unaltered apparently by transmission through the liquid, cannot produce the blue stratum in a posterior solution. Stokes reasoned that certain invisible rays in the beam are changed into visible rays—the blue rays; which means that certain waves of a length too small to be seen are, by incidence on the molecules of the solution, transformed into waves of greater length so as to become visible. How the change takes place is not known; but what Stokes did establish was that the appearance of the visible blue light was due to disappearance of certain invisible light rays. In the substances which Stokes examined, the change was in every case to greater wave-length; on which he based an induction that the change was always from smaller to greater wave-length, an induction which in more recent years has been overturned.

Soon after he contributed to the Cambridge Philosophical Society a paper "On the effect of the internal friction of fluids

on the motion of pendulums." In this he investigates the motion of a pendulum which has for its bob a globe and moves in a viscous fluid contained in a spherical envelope concentric with the bob when at rest; and also the motion of a globe moving uniformly with a small velocity through a mass of viscous fluid. He applies the result of the second investigation to explain the suspension of clouds in the air; and determined from the known viscosity of air the terminal velocity of an exceedingly minute globule of water falling through it. Up to this time the motion of a pendulum had been corrected for buoyancy and for the inertia of the air; Stokes supplied the correction for viscosity.

In 1857 he married, and in consequence of the provision of the statute governing the colleges, his fellowship became vacant. On account of this diminished income he took more work, such as Lectures at the School of Mines in London. When the colleges were reformed (about 1875) fellows engaged in teaching in the University were allowed to retain their fellowships after marriage; and in the case of Stokes the provision was applied extro-actively, and he was reinstated a Fellow of Pembroke College. Professor Stokes not only lectured to the junior members of the University and advised the senior members in questions of applied mathematics, but he was also very helpful to scientists in general. He was in applied mathematics and physics what Cayley was in pure mathematics—a valuable referee and advisor in the work of others. He had the true spirit of a philosopher, more anxious to see science advance than that he should have priority in the advancement. Lord Kelvin has stated that before he removed from Cambridge in 1852, Stokes explained to him the principles of spectrum analysis upon which solar and stellar chemistry has been founded, a work which was afterwards carried out fully by Balfour Stewart and Kirchoff. The following is the account which Stokes himself gives.

In 1849 Foucault accidentally observed that in a solar beam which had traversed the electric arc between two carbon poles, the double dark line *D* appeared darker than usual, and the

bright *D* line was seen in precisely the same place in the spectrum of light coming from the electric arc; Stokes was informed by Foucault of this observation a few years later. It seemed to Stokes that a dynamical illustration of how a medium could act both by emission and absorption for light of a definite refrangibility was not far to seek. He says: "I imagine a series of stretched wires, like pianoforte wires, all turned to the same note. The series, if agitated, suppose by being struck, would give out that note, which on the other hand it would be capable of taking up if sounded in air. To carry out the analogy, we have only to suppose a portion of the molecules constituting the vapor of the arc to be endowed with a capacity of vibrating in a definite manner, that is according to a definite time of vibration. But what were these molecules? It is well known that the bright *D* line in flames is specially characteristic of compounds of sodium, though from its very general, occurrences some had doubted whether it were not really due to something else. But in what condition must we suppose the sodium in the arc to be? The compounds of sodium, such as common salt, carbonate of soda, etc., are colorless; and it would be contrary to the analogy of what we know as to the relation of gases and vapors to their liquids or solutions to suppose that a gas which does exercise absorption should be merely the vapor of a heated solid which does not. On this ground it seemed to me that the substratum which exercised the selective absorption in Foucault's experiment must be free sodium. This might be conceivably set free from its compounds in the intense actions which go on in the sun or in the electric arc; but I had not thought that a body of such powerful affinities would be set free in the gentle flame of a spirit lamp nor experienced that the fact of that flame emitting light of the indefinite refrangibility of *D* entails of *necessity* that it should absorb light of that same refrangibility."

In 1869 Stokes was president of the British Association at a meeting in Exeter. His address was devoted chiefly to recent progress in spectrum analysis to which Mr. Huggins had just applied Döppler's principle in the theory of sound

and deduced that Sirius is receding from the Sun at the rate of 30 miles per second. Stokes closed his address with some observations on life and mind these being characteristic of his philosophical attitude which was that of the golden mean. He says " What this something which we call life may be, is a profound mystery. We know not how many links in the chain of secondary causation may yet remain behind; we know not how few. It would be presumptuous indeed to assume in any case that we had already reached the *last link*, and to charge with irreverence a fellow worker who attempted to push his investigations *yet one step* farther back. On the other hand, if a thick darkness enshrouds all beyond, we have no right to assume it to be impossible that we should have reached even the last link of the chain, a stage where further progress is unattainable, and we can only refer the highest law at which we stopped to the fiat of an Almighty Power. . . . When from the phenomena of life we pass on to those of mind we enter a region still more profoundly mysterious. We can readily imagine that we may here be dealing with phenomena altogether transcending those of mere life, in some such way as those of life transcend, as I have endeavored to infer those of chemistry and molecular attractions, or as the laws of chemical affinity in their turn transcend those of mere mechanics. Science can be expected to do but little to aid us here, since the instrument of research is itself the object of investigation. It can but enlighten us to the depth of our ignorance and lead us to look to a higher aid for that which most nearly concerns our well-being."

In 1880 the Cambridge University Press began the republication in collected form of Stokes' *Mathematical and Physical Papers*. In this publication he introduced for the first time the *solidus* notation for division, originally introduced by De Morgan in his article on the Calculus of Functions in the *Encyclopædia Metropolitana*. If a fraction like $\frac{a}{b}$, or a differential coefficient such as $\frac{dy}{dx}$, is mentioned in the text, the printing of such expres-

sions requires a good deal of " justification " on the part of the compositor. To avoid this expense and the loss of space Stokes introduced the linear notation $a/b$ and $dy/dx$. The symbol: and $\div$ likewise indicate division but he did not use them in the text. He did not use / in writing out centered equations, excepting where it is needed to simplify the index of an exponential function. He considered it convenient to enact that the solidus shall as far as possible take the place of the horizontal bar for which it stands and accordingly that the quantities immediately preceding and following shall be welded into one, the welding action to be arrested by a period. For example $m^2-n^2/m^2+n^2$ is to mean $(m^2-n^2)/(m^2+n^2)$, and $a/bcd$ means $\frac{a}{bcd}$, but $a/bc.d$ means $\frac{a}{bc}d$.

This solidus notation for algebraic expressions occurring in the text has since been used in the Encyclopedia Brittanica, in Wiedemann's Annalen and quite generally in mathematical literature. The solidus may be viewed as a symbol of operation, denoting reciprocal in the same way as $\sqrt{\phantom{x}}$ denotes square root and as $-$ denotes reverse. The expression $/a$ is a sufficient notation for the reciprocal of $a$; in $1/a$ the figure 1 is redundant, just as in $0-a$ the 0 is redundant. The horizontal bar serves the two-fold purpose of a vinculum and a sign for reciprocal. When the reciprocal idea is detached and denoted by /, rules for the manipulation of / can be enunciated; thus $1/a=a$; $(/a)(/b)=1ab$, just as $\sqrt{a}\sqrt{b}=\sqrt{ab}$. The notation of algebra is in fact planar; its complete reduction to a linear form is not a simple matter and was not tackled by Stokes, but this has been attempted by later writers, some of whom write *exp x* for $e^x$. One indeed has proposed to use $\diagdown$ for involution and | instead of a bracket so that $c|(d+e)^3$ would be written $c/|d+e\diagdown 3|$.

In the winters of 1883–4–5 Prof. Stokes delivered in Aberdeen, Scotland, three courses of lectures on Light, under the auspices of the Burnett trust. In 1784 John Burnett, a merchant of Aberdeen, died, bequeathing a portion of his property to establish prizes for the best and next best essay on the following

subject: "That there is a Being, all powerful, wise, and good, by whom everything exists; and particularly to obviate difficulties regarding the wisdom and goodness of the Deity; and, this in the first place, from considerations independent of written revelation of the Lord Jesus; and from the whole to point out influences most necessary for and useful to mankind." The prizes were to be competed for at intervals of forty years; and awards were actually made on two occasions. On account of the length of the interval the trustees began to think that the endowment might be better applied, and they obtained authority to change the funds so as to appoint special lecturers who should be appointed for three years, the courses to be given at intervals of five years and to cover subjects with special regard to the object of the testator. Prof. Stokes was the first lecturer appointed.

The subject of his first lecture was the Nature of Light. He brings out prominently Newton's difficulty in the hypothesis of undulation—that light should produce rays and sharp shadows while sound does not; and Brewsters' difficulty that space should be filled with an ether in order that the light of yon twinkling star may come to us. And he concludes with this lesson: "It may be said, if the former emission theory is nowadays exploded, why dwell on it at all? Yet surely the subject is of more than purely historical interest. It teaches lessons for our future guidance in the pursuit of truth. It shows that we are not to expect to evolve the system of nature out of the depths of our inner consciousness, but to follow the painstaking inductive method of studying the phenomena presented to us, and be content gradually to learn new laws and properties of natural objects. It shows that we are not to be disheartened by some preliminary difficulties from giving a patient hearing to a hypothesis of fair promise, assuming of course that those difficulties are not of the nature of contradictions between the results of observation and experiment and conclusions certainly deducible from the hypothesis on trial. It shows that we are not to attach undue importance to great names, but to investigate in an unbiased manner the facts which lie open to an examination."

In his second course of lectures he treated of light as a means of investigation. One of the objects taken up was the nature of comets. He held that the nucleus consists, in its inner portions at least, of vapor of some kind in an incandescent state. To explain the cause of this incandescence he brings forward the "greenhouse theory." The glass of a greenhouse is transparent to the higher but opaque to the lower forms of radiation, and hence acts as a trap for the sun's rays. The nucleus of the comet he supposed to be surrounded by an envelope of some kind, transparent to the higher but opaque to the lower forms of radiation. Thus solar heat can get freely at the nucleus, but cannot escape until it has raised the nucleus, in part at least, to incandescence. The coma and tail are formed by the condensation of small quantities of this vapor, so that they are mere mists of excessive tenuity. Prof. Tait preferred his own "brickbat theory"; he considered that Stokes' theory made the comet resemble the huge but barely palpable Efreet of the *Arabian Nights*, who could condense himself so as to enter the bottle of brass with the seal of Solomon the son of David. (*Nature*, August 20, 1885.)

The third course of lectures treated of the beneficial effects of light. As regards the special application contemplated by Burnett, he concludes: "If we shut our eyes to the grandeur of Nature and do not attempt, through the things that are made, to acquire higher conceptions of the eternal power and God-hood of the Maker, our conceptions of the Divine Being are apt to become too anthropomorphic. If on the other hand we confine our attention to the study of Nature in all its immensity, our conceptions of its Author are in danger of merging in a sort of pantheistic abstraction in which the idea of personality is lost." Tait remarked with reference to these sentences that the first Burnett lectures had set a noble example to successors, and that Stokes had supplied a valuable warning not only to them but "to the rapidly changing quaternion of neo-teleologists that were soon to be set to work in the Scottish Universities." He referred to the new institution of the Gifford Lecturers.

The second volume of Stokes' *Mathematical and Physical Papers* was published in 1883; the third in 1901. This long delay was due to the fact that his time was engrossed by scientific business; in his later years he seems to have had little ambition in the direction of scientific publication. In 1885 Prof. Stokes after having discharged the duties of Secretary of the Royal Society for thirty years, was elected President, which office he held for the usual period of five years. For twenty years after 1887 he represented the University of Cambridge in Parliament. In 1889 he received the honor of a baronetcy.

In 1891 Sir George Stokes was made one of the changing quaternion to which Tait referred; he was appointed, by the University of Edinburgh, lecturer on the Gifford foundation. Lord Gifford, one of the judges of the High Court of Justice in Scotland, died about 1887, leaving by his will a sum of money to each of the Scottish Universities. The object of the endowment was to appoint for one or two years a thinker, who might not belong to any Christian denomination provided only he was a true and reverent inquirer after truth, to deliver a course of public lectures on some point bearing on Natural Theology, treating the subject just as any other science. Stokes delivered two courses of lectures in 1891 and 1893. Trained in Cambridge University where little attention was paid to philosophy, he seems to have felt a difficulty in treating Natural Theology " just as any other science " and he could not speak with the same authority as when he was discoursing on light.

Four years ago (in 1899) the University of Cambridge celebrated in brilliant style the jubilee of his professorship. Delegates were invited from the learned societies; sixty-eight of them, mostly British, were represented. The celebration of the jubilee began with the delivery of the Rede lecture by Prof. Cornu of the école polytechnique of Paris; the endowment for this lecture dates back to 1524. The subject was appropriate to the occasion: " The wave-theory of light, its influence on modern physics." At an evening reception a bust of Stokes was presented to Pembroke College and a replica to the University. The presentation was made by Lord Kelvin who

said that Sir George Stokes had published in his own name but a very small part of the good he had done to the world. At the principal function, which was held in the Senate house, the delegates were received by the Vice Chancellor of the University; they presented the addresses of which they were bearers and these were handed to Sir George. In reply he said that he often thought, in reviewing his long life, that he might have worked harder, and he attributed his longevity to his comparative idleness—a remark which was cheered by the undergraduates in the gallery. A special meeting of his early love, the Cambridge Philosophical Society, was held and the papers there presented are published in a memorial volume.

In the summer of 1902 he was elected to the mastership of Pembroke College. Later in the year he took part with Lord Kelvin in making the presentation of a portrait of Prof. Tait to St. Peter's College. He died on February 1, 1903, in the 84th year of his age. In many respects the life of Stokes resembles that of Newton. Both were skilled experimenters, especially in optics; of Stokes it used to be said that if you gave him sunlight and three-quarters of an hour, there was no experiment in optics he could not perform. Both Newton and Stokes filled the Lucasian chair of mathematics; both represented Cambridge University in Parliament; both filled the offices of Secretary and President of the Royal Society; both received the dignity of *Sir*; and both lived to an advanced age. They also resembled one another in type of mind and in religious views; but Stokes never sat down to produce a work at all commensurate in labor or in importance with the *Principia.*

## SIR GEORGE BIDDELL AIRY *

(1801–1892)

George Biddell Airy was born at Alnwick in Northumberland on the 27th of July, 1801. His father, William Airy, was collector of the Excise duties for that district; his mother, Anna Biddell Airy, was the daughter of George Biddell, a well-to-do farmer in Suffolk. In 1810 William Airy was transferred to the county of Essex, and the family then settled in Colchester, the county town. Here George was first sent to a private school, where he got a good introduction to elementary mathematics; afterwards he was sent to the grammar school where he was initiated in Latin and Greek to the extent of being able to write Latin prose. He also got the usual instruction in Latin verse, but he did not excel in that kind of composition. On one occasion his father brought him a present from London, which had much influence on his future career—a terrestrial and a celestial globe. From this event he dated his interest in astronomy. Arthur Biddell, his mother's brother, lived on a farm at Playford, in Suffolk, and was a man of some scientific and literary culture, besides being interested in historical and antiquarian matters. George spent his holidays in this uncle's company and especially in his library; from this source originated an interest in mechanics, optics, poetry and antiquities. There he found the means of constructing a telescope for himself.

At school he distinguished himself, especially in memory work. Although not wanting in courage, he did not take an interest in athletic sports. It was the custom for each boy once a week to repeat a number of lines of Latin or Greek

* This Lecture was delivered on April 7, 1904.—Editors.

poetry, the number depending very much on his own choice. Airy repeated 100 lines every week; on one occasion he repeated more than 2000 lines. The schoolmaster was impressed with his powers and suggested to his father that the boy should be sent to Cambridge; who, on inquiry, concluded that the expense was too great for his straitened circumstances. However, the uncle took up the problem, and with the help of a Cambridge alumnus got the boy prepared in classics and mathematics for the entrance examination to Trinity College. In these preliminary examinations he acquitted himself so well, that a reputation for scholarship preceded his going there to reside. In 1819, when 18 years old, he commenced residence as a sizar of Trinity College. By a *sizar* is meant a poor student who is exempted from some of the expenses—he does not pay for dinner in hall; the sizars dine after all the rest, on the remains of the Fellows' dinner. Newton himself started in that same college as a sizar. George Peacock, who was then a mathematical tutor of the college, became his warm friend and adviser; he gave him a copy of Lacroix's *Differential Calculus*, translated by himself, Babbage, and Herschel, and also a copy of his *Collection of Examples*. At this time the Differential Calculus was beginning to prevail over Fluxions; Airy had got instruction in the old method, but he took to the new with great industry. At a breakfast party at Peacock's he met Whewell, who was a resident fellow graduate of the University.

Airy employed part of his first vacation in writing out a paper on the geometrical interpretation of $\sqrt{-1}$. He got the suggestion of "perpendicular" from some book; the aim of his essay was to apply that theory. Peacock to whom he showed the essay was much pleased. Mr. Hustler, his tutor, on the contrary disapproved of his employing his time on such speculations. The former was a philosopher and reformer, the latter an official and disposed to consider everything that is, is right. Airy however, whether by the influence of Hustler or otherwise, did not go very deep into the subject. He afterwards wrote: "I have not the smallest confidence in any result which is essentially obtained by the use of imaginary

symbols. I am very glad to use them as conveniently indicating a conclusion which it may afterwards be possible to obtain by strictly logical methods; but until these logical methods shall have been discovered, I regard the result as requiring further demonstration." Here we note a want of confidence in mathematical deduction which appears to have been characteristic of Airy and his generation of mathematicians.

In his first year Airy read Whewell's textbook on Mechanics, just published, the first innovation made in the Cambridge system of Physical Science for many years, and which made partial use of the differential notation (*d*). By the beginning of his second year he was so far advanced that he took two private pupils for instruction in mathematics—men of his own year. By this means he became able to defray all his expenses without help from his relatives. In his early career as a student he started the custom of keeping on his desk a quire of scribbling paper sewed together; and on the current quire everything was written—translations from the Greek, prose translations into Latin, mathematical problems, memoranda of every kind. These quires were carefully preserved. This habit of writing out everything made him an accurate and ready man, and placed him far ahead of his contemporaries in the college examinations. He adopted the rule of writing on his quire every day a translation into Latin of three or four sentences; this he did in preparation for the final University examinations. While he was an undergraduate Babbage's difference machine was much talked of: in his last undergraduate year (1822) Airy studied the subject and made a sketch of a computing machine. About the same time he prepared a paper on the construction of a reflecting telescope with silvered glass; a paper which brought him an introduction to Mr. (afterwards) Sir John Herschel, and Mr. (afterwards) Sir James South, two of the active astronomers of the day.

In Airy's time a candidate for B. A. was required to pass a University ordeal, which was a survival of the ancient system of examination. The candidate at the end of his second and third years was required to state three theses which he was

prepared to defend in Latin against as many opponents. For instance Airy submitted the following theses:

(1) Recte statuit Newtonus in Principiis suis Mathematicis, libro primo, sectione undecima.

(2) Recte statuit Woodius de Iride.

(3) Recte statuit Paleius de Obligationibus.

An apponent was appointed to attack each thesis. The discussion was carried in the Latin language under the direction of a Moderator; and when the high men were engaged, the spectacle was sufficiently interesting to draw a great crowd of undergraduates. The statutes framed in the time of Queen Elizabeth, required that a candidate should keep a certain number of such acts; at this time all excepting the two mentioned were gone through as a matter of form. Airy's practice in Latin enabled him to acquit himself with high distinction. A few years later the respondent and opponents reduced the procedure to a farce by concocting their arguments beforehand, and the system was suppressed in 1830. This procedure explains the term *wrangler* and *senior wrangler*; the contest was originally a wrangle in the Latin language. In Airy's time there was a further tripos examination conducted partly in writing, partly viva voce in English. Airy came out Senior Wrangler, very far ahead of the next man. The year before Peacock had introduced a paper of questions entirely on the Differential Calculus, a procedure which definitely established the study of the continental mathematics at Cambridge University.

After graduating as B.A., Airy continued to read for the fellowship examination, and to take pupils, generally four in number. He was now elected a member of the Cambridge Philosophical Society. During the vacation he went on a geological tour in Derbyshire, visiting among other places Edensor, near Chatsworth, the principal residence of the Duke of Devonshire. His introduction was to the rector, the Rev. Richard Smith, a Cambridge man; he fell in love with the eldest daughter, and within two days proposed an engagement to marriage. This was before he entered the competition for fellowship, and in view of the rules then in force about the

tenure of fellowship, was a rather bold step. No engagement was then made. In 1824 he was elected a fellow of his college. He also obtained the post of assistant mathematical tutor, and in addition took some private pupils. While engaged in this work he prepared a volume called *Mathematical Tracts*, on subjects which were either deficient at the University, or else not presented in readable form, namely, Lunar Theory, Figure of the Earth, Precession and Nutation, and the Calculus of Variations. The volume was printed by the University Press, and brought its author both reputation and some money. This book, published in 1826, applied the continental notation of the calculus and it exerted a great influence on the study of mathematical physics at Cambridge.

Whewell was senior to Airy in academic standing by seven years. In 1826 they made experiments on gravity in the Dolcoath mine in Cornwall. One pendulum was swung at the top of the mine, the other at the bottom. After numerous observations of their periods in these positions, the one down below was sent up to be compared with the other at the top; when it emerged at the top, the experimenters were surprised and mortified to find the basket on fire, and hence the observations had to be abandoned. This same year the Lucasian professorship of mathematics fell vacant; a Head of one of the colleges sought to capture it as a sinecure; Charles Babbage, who had taken only a poll degree at Cambridge, also applied; and so did Airy. Babbage and the Head mutually destroyed one another, with the result that Airy was elected. Airy improved his academic standing, but not his income; the salary was only £100, and the position involved the giving up of some tuition work. He was not yet in a position to sacrifice his fellowship by marriage. He immediately issued a printed notice that he would give professional lectures in the next term. There had been no lectures on Experimental Philosophy (Mechanics, Hydrostatics, Optics) for many years. The University in general looked with great satisfaction on such vigorous reform; but there were difficulties to surmount; no allotted term for the lectures, no allotted hour of the day, scarce any available

lecture-room. In this contest Airy and Babbage first came into conflict.

It was the next year (1827) that Airy's path first intersected that of Hamilton. Dr. Brinkley, the professor of astronomy at Dublin had been made a bishop. Airy went over to Dublin to see about the appointment: finding that the electors desired to appoint W. R. Hamilton, although still an undergraduate, he retired. The following year the Plumian professorship of astronomy and experimental philosophy at Cambridge fell vacant, the salary of which was £300. Airy applied, and before he was elected took the extraordinary course of applying for an increase of salary. He was anxious to secure an income on which he could marry—a difficult thing in the constitution of the University. His good fortune did not fail him; he was elected and the salary raised to £500. He had now charge of the College Observatory, and a residence, to which two years later he brought Richarda Smith from Edensor. For eight years he lived and worked in the Cambridge Observatory. One of his first scientific works was a repetition along with Whewell of the pendulum experiments in the Dolcoath mine. Misfortune again attended the inquiry. A few days after the observations had been started, a mass of rock settled in the mine, stopping the pumps and allowing the water to accumulate; sufficient time was not left to complete the observations, and the result was again nugatory. After one year at the Observatory Airy began to publish his astronomical observations, first of all devising an orderly system of exhibition, then " quite a novelty in astronomical publications."

In 1832 a committee of the newly founded British Association asked Airy to prepare the report on Astronomy for the next meeting to be held at Oxford. This he did, and read part of it at the meeting. Mr. Vernon Harcourt, secretary of the Association, deprecated the tone of the report as relating to English astronomers; but Airy refused to alter a word. About this time Sir James South, the astronomer, on removing to a house in Kensington, bought a 12-inch achromatic telescope in Paris and employed Troughton & Simms of London to mount it

equatoreally. South was not satisfied with the work, and refused to pay, and a lawsuit followed in which the English astronomers of the day were called on as expert witnesses. Airy and Sheepshanks were on the side of the contractors; Babbage on the side of South. The court appointed an arbitrator, who decided against South; whereupon he dismantled the telescope and issued the following notice:

OBSERVATORY, KENSINGTON

To shyock toymakers, smokejack makers, mock coin makers, etc. Several hundred weights of brass etc., being the metal of the great equatoreal instrument made for the Kensington Observatory by Messrs. Troughton & Simms, are to be sold by hand on the premises; the wooden polar axis of which, by the same artists, with its botchings cobbled up by their assistants, Mr. Airy and the Rev. R. Sheepshanks, was purchased by divers vendors of old clothes, and dealers in dead cows and horses, with the exception of a fragment of mahogany specially reserved at the request of several distinguished philosophers, on account of the great anxiety expressed by foreign astronomers to possess them, was converted into snuff boxes as a souvenir piquant of the state of the art of astronomical instrument making in England during the nineteenth century.

This dispute occasioned by one who eventually proved to be insane, led to much quarreling among the astronomical scientists of the day. De Morgan as a friend of Airy and Sheepshanks was publicly insulted by South, and on asking an explanation from him received what was virtually a challenge to a duel. Babbage, on the other hand, by his support of South, inflicted much damage on his own career. South, who was on the board of visitors, attacked Airy's administration of the observatory in public.

In 1835 Airy received an exceptional favor from the British Government; a pension of £300 was settled on his wife. Airy was a liberal, the Government conservative. No personal or political obligation was imposed; it was given avowedly as an encouragement to science. Later in this year a liberal Government came into power; they offered him the appointment of astronomer royal at the Greenwich Observatory, which

at that time had fallen into a very inefficient state. He accepted and then they offered him Knighthood, which he declined on the ground of not being wealthy enough. When Airy took charge of the Cambridge Observatory, it had only one instrument—a transit instrument, and no assistant. By the date when he left for Greenwich the University had erected a Mural Circle and a small Equatoreal, and he had induced the Duke of Northumberland—a great patron of science—to purchase and erect what was then the finest equatoreal telescope in England.

At Greenwich Observatory Airy appointed two new assistants, and he speedily introduced his system of order. He introduced thirty printed skeleton forms for observations and computations; procured a copying press; punched four holes in papers and tied them flat in packets and subordinate packets. Later he got from a manufacturer a machine to punch the holes; and his system was an anticipation of the device which is now common in offices. All papers were carefully preserved in their proper place; and in his later years the ruling passion for order was so great, that he took more pains to classify a letter properly than to master its purport. About this time the difficulty of navigating iron ships was pressed on the Government; they asked Airy to make experiments on a ship. He made a series of observations, reduced them, and prepared magnets and iron correctors to neutralize the disturbance mechanically. He was successful in substituting mechanical for tabular correction; but the sluggishness of the large magnet of the compass remained a difficulty. Subsequently Sir William Thomson introduced instead of the large magnet a number of small magnets, and put a patent on it; but Airy got nothing from the Government for solving the main part of the problem. Being a very methodical man Airy kept a diary. Under September 15, 1842 he entered the following: "The Chancellor of the Exchequer asked my opinion on the utility of Babbage's calculating machine, and the propriety of expending further sums of money on it. I replied, entering fully into the matter, and giving my opinion that it was worthless."

Fortified with this opinion, the Government broke off definitely with poor Babbage.

Airy's successor at the Cambridge Observatory was named Challis. In 1844 Prof. Challis introduced to Airy by letter the senior wrangler of the previous year named J. C. Adams, who in consequence of having read Airy's report on recent progress in astronomy to the British Association had several years before formed the design of investigating the unexplained irregularities in the motion of the planet Uranus, and who was now, his undergraduate years over, busily engaged on the solution. Adams wished to be furnished with the Greenwich observations of Uranus; these were promptly supplied. A year later Challis wrote a letter of introduction to Airy beginning: "My friend Mr. Adams, who will probably deliver this note to you, has completed his calculations respecting the perturbation of the orbit of Uranus by a supposed ulterior planet, and has arrived at results which he would be glad to communicate to you, if you could spare him a few moments of your valuable time." Provided with this letter, Adams called at the Royal Observatory; Airy was absent in France. A month later, when Airy had returned, Adams called again; the astronomer royal was taking his midday walk, but would be back soon. Adams called an hour later; the astronomer was at dinner, and granted no interview. Adams left a paper giving the results of his investigation—the mass, position, and elements of the orbit of the new planet. A few days later Airy sent him a letter inquiring whether his theory likewise accounted for the irregularities in the radius-vector of Uranus. Adams did not reply; he felt mortified, and he thought the question trivial. Airy wrote no further letter to Adams; but a few months later, when Leverrier communicated similar results in a letter, he replied hailing Leverrier as the true predicter of the new planet. It was Airy's custom to turn off visitors without seeing them; interviews interfered too much with his pet order. He forgot his official position, and how he himself had been assisted. Adams was very unfortunate in the man to whom he confided his results. Prof. Challis made use of the Cam-

bridge telescope to search for the planet; but he was anticipated by a Berlin astronomer who followed Leverrier's prediction. Challis actually mapped the planet as a star twice, but had not compared his maps. A great controversy arose; the attitude is neatly expressed by the couplet:

> When Airy was told, he wouldn't believe it;
> When Challis saw, he couldn't perceive it.

In the early forties there raged in England the "battle of the gauges." Of the railroads built some had adopted a broad gauge (6 feet), some a narrow gauge (4 feet $8\frac{1}{2}$ inches). The inconveniences of the diversity were beginning to be felt acutely, and the Government appointed a commission of which Airy was a member. The commissioners reported in favor of the universal use of the narrow gauge; their recommendation was opposed effectively in Parliament by the broad gauge interest, supported by Babbage, who devised very ingenious instruments and made much more scientific observations than Airy. However the narrow guage gradually became the solution of the difficulty. In the fall of 1848 Lord Rosse invited a number of astronomers to his castle at Parsonstown, Ireland, in order that they might inspect his large reflecting telescope. They were entertained for two weeks, Airy and Hamilton were the principal experts. Airy was able to remove a fault in the mounting of the great mirror, for in practical astronomy he was immensely superior to Hamilton; but as a calculator and scientific genius Hamilton was as much superior. It was on this occasion that Hamilton, influenced by Airy's sarcastic remarks, broke his abstinence resolution.

In 1851 Airy presided over the British Association, at the meeting held in Ipswich. The next year he communicated a paper to the Royal Society on the "Eclipses of Agathocles, Thales, and Xerxes." And he also lectured on the subject at the Royal Institution. In 1854 he renewed the attempt to determine by pendulum vibrations the intensity of gravity at the bottom of a mine; this time he chose the Harton coal mine in the north of England, and for his assistants observers

from the different astronomical observatories of the country. The observations were successful; they gave the result that gravity is increased at the depth of 1260 feet by 1/19000th part: from which he estimated the density of the Earth to be 6.565. Airy not only supplied Hansen with the Greenwich observations of the Moon for the purpose of constructing his Lunar Tables, but he had them printed at the expense of the British Government and secured for him a personal grant of £1000 against the opposition of Babbage and South, who were on the Board of Visitors for the Observatory.

Airy came into conflict with Prof. Cayley about the kind of questions that ought to be set at the Cambridge Tripos Examination. Airy held that " The papers were utterly perverted by the insane love of problems, and by the foolish importance given to wholly useless parts of algebraical geometry. For the sake of these every physical subject and every useful application of pure mathematics was cut down or not mentioned." When invited to make an address at Cambridge, he seized the occasion to renew the attack; he also wrote to the board of mathematical studies. He wished to introduce into the list of subjects for examination Partial Differential Equations, Probabilities, Mechanics in a form which verges on practical application, Attractions, Figure of the Earth, Tides, Theory of Sound, Magnetism but not (for the present) Mathematical Electricity. In the correspondence which followed Cayley said, " I think that the course of mathematical study at the University is likely to be a better one if regulated with a view to the cultivation of science as if for its own sake, rather than directly upon consideration of what is educationally best (I mean that the best educational course will be so obtained) and that we have thus a justification for a thorough study of pure mathematics. In my own limited experience of examinations the fault which I find with the men is a want of analytical power, and that whatever else may have been in defect Pure Mathematics has certainly not been in excess." Later Airy criticized the questions set for the Smith prizes in 1879 in a letter addressed to the members of the Senate. He singled

out the following question, "Using the term circle as extending to the case where the radius is a pure imaginary, it is required to construct the common chord of two circles." This drew forth from Cayley a rejoinder in which he gave a solution of the problem. To which Airy replied, "I am not so deeply plunged in the mists of impossibles as to appreciate fully your explanation in this instance, or to think that it is a good criterion for University candidates." The dispute ended in the introduction of mathematical physics into the course of study.

Airy was a liberal in religious attitude. He sympathized with the agitation which led to the abolition of religious tests for M.A. degree at the Universities of Oxford and Cambridge. He also supported his fellow mathematician Colenso when he was attacked for his writings on the Pentateuch. With respect to Colenso he wrote, "He has given me a power of tracing out truth to a certain extent which I never could have obtained without him. And for this I am very grateful. As to the further employment of this power he and I use it to totally different purposes. But not the less do I say that I owe to him a new intellectual power." During the years 1872–3 Airy was president of the Royal Society. In 1872 he was knighted by Queen Victoria; he had declined the honor three times before. In 1873 he was consulted by Barlow and Bouch the engineers for the construction of a railway bridge across the Firth of Tay, on the subject of the wind pressure that should be allowed for. This bridge was blown down in 1879 with a passenger train on it, no one surviving to tell the tale. Airy's report was subsequently much referred to at the official inquiry into the causes of the disaster.

In 1881 when 80 years old (20 years over the limit assigned by Osler for good work!) Airy resigned the office of astronomer royal and the Government, on account of his exceptional public services, granted him a pension almost equal to his salary. He died on the 2d of January, 1892 in the 91st year of his age. His life had been one of great activity; he was the author of eleven volumes and of 518 papers extending from 1822 to 1887. With regard to his habits while he was at Green-

wich Observatory, he generally worked in his office from 9 to about 2:30, then took a walk, dined at about 3:30, and afterwards slept for about an hour. In the evening he worked in the same room with his family. "His powers of abstraction were remarkable; nothing seemed to disturb him, neither noise, singing nor miscellaneous conversation. . . . With his natural love of work and with the incessant calls upon him, he would soon have broken down, had it not been for his system of regular relaxation. Two or three times a year he took a holiday, generally a short run of a week or ten days in the spring, a month in the early autumn and about three weeks in the winter." Airy did valuable work and exerted great influence; especially we may look upon him as the founder at Cambridge of the modern school of mathematical physics.

# JOHN COUCH ADAMS *

## (1819–1892)

JOHN COUCH ADAMS was born on the 5th of June, 1819, at a farmhouse seven miles from Launceston in Cornwall. His father was a tenant farmer, and so had been his ancestors for several generations. His mother, née Tabitha Knill Grylls, owned a small estate, inherited from an aunt named Grace Couch; hence the middle name of the mathematician. John Couch Adams was the oldest of seven children; he had three brothers and three sisters; his brother William Grylls Adams became a professor of physics, and has attained to scientific distinction although not comparable to that of his brother. Adams was thus of the old Welsh stock located in the south-western peninsula of England. He received his primary education at the village school near the farm, where at ten years of age he studied algebra. In his own home there was a small library, which also had been inherited by his mother, and which included some books on astronomy. He constructed a simple instrument to determine the elevation of the sun. " It consisted of a vertical circular card with graduated edge, from the centre of which a plumb bob was suspended. Two small square pieces of card, with a pinhole in each, projected from the circular disc at right angles to its face at opposite ends of a diameter. The card was to be so placed that the sun shone through the pin holes, and the elevation was read off on the circle."

At twelve years of age he was placed in a private school taught by the Rev. John Couch Grylls, a cousin of his mother, the subjects of instruction being classics and mathematics. Here

* This Lecture was delivered on April 8, 1904.—EDITORS.

he had access to a public library, where he studied more books on astronomy, and also Vince's *Fluxions*, then the principal testbook at Cambridge on the higher mathematics. Thus early was he introduced to Newton's methods. While at this school he watched for three weeks for a predicted return of Encke's comet; at last he saw it (1835) and he wrote home, "You may conceive with what pleasure I viewed this, the first comet which I had ever had a sight of, which at its visit 380 years ago threw all Europe into consternation, but now affords the highest pleasure to astronomers by proving the accuracy of their calculations and predictions." The following year an annular eclipse of the Sun took place. For the people on the farm he made a calculation of the times of the eclipse for that meridian and latitude, and also a diagram of the eclipse as it would appear to them. Next year his account of observations of an eclipse appeared in the London papers. He was now 18 years old, and had shown such signs of mathematical power that preparations were made to send him to Cambridge.

In 1839, when 20 years old, he entered St. John's College; while an undergraduate he was invariably the first man of his year in the college examinations. It was his custom to keep a memorandum book, in which at the end of his second college year (July 3, 1841) he made the following entry: "Formed a design, in the beginning of this week, of investigating as soon as possible after taking my degree, the irregularities in the motion of Uranus, which are yet unaccounted for, in order to find whether they may be attributed to the action of an undiscovered planet beyond it; and if possible thence to determine the elements of its orbit, etc., approximately, which would probably lead to its discovery." His attention had been drawn to the phenomenon by reading Airy's report on Astronomy to the British Association (1831–2); but no explanation is suggested there. Meanwhile he kept to the beaten path of training for the Tripos; as a result in 1843 he won the first place in that examination, the first Smith's prize and a fellowship from his college. After taking his degree, Adams attempted a first solution of his problem on the assumption that the orbit

was a circle with a radius equal to twice the mean distance of Uranus from the Sun—an assumption suggested by Bode's law. The result showed that a good general agreement between his theory and observation might be obtained. He now in 1844, if not before, acquainted Prof. Challis, Airy's successor with his scientific enterprise; and through him made a request to Airy for the errors of the tabular geocentric longitude of Uranus for 1818–26, with the factors for reducing them to errors of heliocentric longitude. Airy at once supplied all the results of the Greenwich observations of Uranus from 1754 to 1830.

With these improved data Adams now undertook a more elaborate discussion of the problems, retaining however the former assumption with respect to the mean distance; and by September of the following year (1845) he had the investigation completed. He communicated the results to Prof. Challis in the form of a note giving numerical values for the new planet, of its mean longitude at a given epoch, the longitude of its perihelion, the eccentricity of its orbit, its mass and its geocentric longitude for the last day of the month but without any account of his method. Challis on the 22d of September wrote to Airy a letter to introduce Adams; the first sentence in that letter has been already quoted in my lecture on Airy. Challis further said that he considered Adams' deductions to be made in a trustworthy manner. Challis had the best facilities in England to search for the predicted planet, yet he turned the matter over to Airy. Provided with the letter Adams called at the Greenwich Observatory, and met with the experiences described in my last lecture. Adams was naturally of a shy disposition and he felt mortified. In reply to the paper of results that he had left at the Observatory, Airy sent, a fortnight later, a letter to Adams: "I am very much obliged by the paper of results which you left here a few days since, showing the perturbations on the place of Uranus produced by a planet with certain assumed elements. . . . I should be very glad to know whether this assumed perturbation will explain the error of the radius-vector of Uranus." The prin-

cipal result was that the mean longitude of the planet for 1st of October, 1845, was 323° 34′. Adams was hurt at the reception which his results had obtained; regarding Airy's question as of trifling importance he did not send any answer immediately but applied himself to a new calculation on the assumption of a smaller mean distance.

That same November a French astronomer, M. Leverrier, presented a paper to the French Academy on the perturbations of Uranus produced by Jupiter and Saturn, and concluded that these were quite incapable of explaining the observed irregularities. In June of the next year he presented his second paper which showed that there was no other possible explanation of the discordance, except that of an exterior planet. Further, like Adams, he assumed the distance to be double that of Uranus, and calculated that its longitude at the beginning of the next year (1847) would be 325°. Leverrier communicated his results by letter on the 24th of June to Airy, who on comparison found that there was only about one degree of difference in the predicted places of Adams and Leverrier. The next day (June 29) a meeting of the Board of Visitors took place at the Greenwich Observatory; Sir John Herschel and Prof. Challis were present as visitors. In the course of a discussion, Airy referred to the probability of shortly discovering a new planet, giving as his reason the close coincidence of Adams' and Leverrier's predictions. Early in July Airy thought it time that a search should be made for the planet. He considered the Cambridge telescope the best for the purpose, and he asked Prof. Challis whether he would undertake it, and the latter agreed to do so. Airy suggested the formation of three successive maps of the stars down to the 4th magnitude, in a band of the heavens 30° long by 10° wide having the predicted place of the planet as its centre. When the successive sets of observations were mapped, the planet could be detected by its motion in the interval.

At the end of August Leverrier presented his third memoir to the French Academy in which he gave the calculated elements of the orbit of the planet. He also restricted as far as possible

the limits within which the planet should be sought; he predicted that it would have a visible disc, and sufficient light to make it conspicuous in ordinary telescopes. By this time Adams had completed his new investigation on the assumption of a distance 1/30 less than before; the results agreed still better with observation. In a letter to Airy he communicated the new results, answered his question about the errors of the radius-vector, and intimated that he was thinking of presenting a brief account of his investigation at the coming meeting of the British Association. Airy at this time was again absent on the Continent; the British Association met; Adams came with his paper, but the section of mathematics and physics had adjourned the day before he arrived. Had he been present at the beginning of the meeting he would have heard Sir John Herschel say in his address on resigning the chair to his successor, after referring to the astronomical events of the year, which included a discovery of a new minor planet: "The year has done more. It has given us the probable prospect of the discovery of another planet. We see it as Columbus saw America from the shores of Spain. Its movements have been felt, trembling along the far-reaching line of our analysis, with a certainty hardly inferior to that of ocular demonstration."

In this same month of September Leverrier sent his predictions to Dr. Galle of the Berlin Observatory in a letter received September 23, 1846. Dr. Galle was already provided with a map of the part of the heavens prescribed, and that very evening he found a star of the eighth magnitude which did not exist on the map; observation on the following evening showed that its motion was nearly the same as that of the predicted planet. On October 1st Challis heard of the discovery of the planet at Berlin. He then found that he had actually noted it on August 4 and August 12, the third and fourth nights of his search, so that had the observations been compared as the work proceeded, the planet might have been discovered by him before the middle of August. The discovery of the planet by Dr. Galle, in consequence of Leverrier's prediction, was received with the greatest enthusiasm by astron-

omers of all countries, and in France the planet was at once called Leverriers' planet or even "Leverrier." Sir John Herschel was the first to speak for Adams. He wrote a letter to the *Athenæum* in which he recalled his works at the Southampton meeting, and explained that the ground of his confidence was the near coincidence of the results of two independent investigations—that by Leverrier, and another by a young Cambridge mathematician named Adams. He invited Adams to place his calculations in full before the public; this Adams did on the 13th of November, 1846, in a memoir read before the Royal Astronomical Society.

At the time of Galle's discovery Airy was on the Continent. On returning to Greenwich he wrote to Leverrier (October 14, 1846), "I was exceedingly struck with the completeness of your investigations. May you enjoy the honors which await you! and may you undertake other work with the same skill and the same success, and receive from all the enjoyment which you merit! I do not know whether you are aware that collateral researches had been going on in England, and that they had led to precisely the same result as yours. I think it probable that I shall be called on to give an account of these. If in this I shall give praise to others, I beg that you will not consider it as at all interfering with my acknowledgment of your claims. You are to be recognized beyond doubt as the real predicter of the planet's place. I may add that the English investigations, as I believe, were not quite so extensive as yours. They were known to me earlier than yours." Leverrier naturally felt much hurt by Herschel's article and Airy's letter. He could not understand why Adams had not published his results. Other French astronomers were at first very unwilling to admit that Adams had any rights whatever in connection with the planet, but later, at the suggestion of the great French astronomer Arago, the name Neptune was adopted and has since been universally used. It was now time for Prof. Challis to publish what he knew of the matter. He gave in the *Athenæum* for October 17 an account of Adams' investigations, and it was then publicly known for the first time that Adams' con-

clusions had been in the hands of Airy and Challis since 1845, and that Challis had actually been engaged in searching for the planet. The British astronomers were divided in opinion; some held that the fact that Adams' results had not been publicly announced deprived him of all claims in relation to the discovery. The Royal Society of London rather hastily (1846) awarded it highest honor, the Copley medal, to Leverrier alone; and in the Royal Astronomical Society a majority of the Council were in favor of awarding their gold medal to him; but a sufficient minority of the Council protested. Two years later the Royal Society made some amends by awarding the Copley medal to Adams.

In 1847 the Queen with Prince Albert visited the University of Cambridge; on this occasion the honor of knighthood was offered to Adams, then 28 years old, but he felt obliged to decline for the same reason as Airy had done before. The members of St. John's College, in honor of the brilliant achievement of one of their number founded the Adams prize, to be awarded biennially for the best essay on some prescribed subject in pure or applied mathematics; its value is about £225. In this year also, Prof. Benjamin Pierce of Harvard College published a paper in which he criticized the methods of Adams and Leverrier, contending that the period of Neptune differed so considerably from that of the hypothetical planet that the finding of the planet was partly due to a happy accident. Adams, on the occasion of the republication of his memoir in Lionville's *Journal* in 1877, replied that the objection did not hold on account of the perturbations considered lying within a fraction of the synodic periods of Neptune and Uranus. In this year Leverrier attended the meeting of the British Association at Oxford, in the company of Airy. The two discoverers of Neptune met then, and ever after manifested a high appreciation for each other. In 1876 when Adams was president of the Royal Astronomical Society he made an address on presenting a second gold medal to Leverrier for his theories of the four great planets, Jupiter, Saturn, Uranus, and Neptune.

Adams was by nature a calculator, not an observer or experi-

menter. Hence it is not surprising to find that his next research work was the determination of the constants in Gauss' theory of terrestrial magnetism—a subject to which he devoted much time in his later years, and which he left unfinished. In 1851 Adams was elected president of the Royal Astronomical Society. In 1852 his fellowship at St. John's College expired, because he had not taken clerical orders; he was however elected to a fellowship at Pembroke College, which he retained till his death. In 1853 Adams communicated to the Royal Society his celebrated memoir on the secular acceleration of the Moon's mean motion. Halley was the first to detect this acceleration by comparing the Babylonian observations of eclipses with those of Albatagnius and of modern times, and Newton referred to his discovery in the second edition of the *Principia.* The first numerical determination of the value of the acceleration is due to Dunthorne, who found it to be about 10″ in a century. Laplace was the first to deduce the acceleration theoretically from Newtonian principles; the result is given by an infinite series of which he calculates only the first term. Plana, an Italian mathematician, found the next term to be $-\frac{2187}{128}m^4$; Adams by his investigation found it to be $-\frac{3771}{64}m^4$, which reduced the value of the first term from 10″ to 6″. This paper gave rise to a violent controversy; those opposed holding that the result was contradictory to observation. But Adams was safe; his result depended entirely on algebraical considerations—on the solution of a differential equation, not on observation; consequently his result finally prevailed.

In 1858 Adams' life at Cambridge was interrupted; he was appointed professor of mathematics in the University of St. Andrews, Scotland. At the end of a year he returned to Cambridge as Lowndean professor of astronomy and geometry. As Lowndean professor he lectured during one term in each year, generally on the lunar theory, but sometimes on the theory of Jupiter's satellites, or the figure of the Earth. Two

years later he succeeded Challis as the Director of the Cambridge Observatory and settled down as a married man. Henceforth the center of his scientific activity was the Observatory house, where Airy and Challis had lived, situated on an eminence about a mile west of Cambridge on the Huntington road. The observatory was well equipped, thanks to Airy's efficient incumbency; but Adams was by nature a calculator, and the instruments were not much used during his tenure of office.

In 1866 Adams took up the problem of the November meteors, drawn thereto by the remarkable display of that year. Prof. Newton of Yale had published a memoir in the *American Journal of Science and Arts* in which he collected and discussed the original accounts of thirteen displays of these meteors in years ranging from A.D 902 to A.D. 1833; he inferred that these displays recur in cycles of 33.25 years, and that during a period of two or three years at the end of each cycle a meteoric shower may be expected. He concluded that the most natural explanation of these phenomena is, that the November meteors belong to a system of small bodies describing an elliptic orbit about the Sun, and extending in the form of a stream along an arc of that orbit which is of such a length that the whole stream occupies about one-tenth or one-fifteenth of the periodic time in passing any particular point. He showed that in one year the group must have a periodic time of either 180.0 days, 185.4 days, 354.6 days, 376.6 days or 33.25 years. Prof. Newton found that the node of the orbit of the meteors is gradually increasing; that the rate is $52''.4$ with respect to the fixed stars; and he remarked that with this datum and the position of the radiant point, computation might be able to determine which of the five periods is the correct one. He considered 354.6 days the most probable. Adams then took up the problem. He found that none of the first four periods satisfied the data, while the fifth one of 33.25 years did. He concluded that he had settled the question of the periodic time of the November meteors beyond a doubt. The elements of their orbit obtained by Adams agreed very approximately with those of a comet

observed in 1866, and it seemed probable that the meteors and the comet constituted one moving aggregation. In 1899, thirty-three years later, an exceptional display of meteors was predicted on the strength of Adams' result; there was much popular lecturing on the subject beforehand; the citizens of London on the predicted night went to bed having previously arranged with the policeman on the beat to call them up, but their slumbers were not disturbed.

Eleven years later (1877) Adams recognized the merits of an American astronomer George W. Hill, who was then an assistant in the office of the *American Nautical Almanac*, and whose eminence as an astronomer is now universally recognized in the world of science. Hill in 1877 published a paper on the motion of the moon's node in the case when the orbits of the Sun and Moon are supposed to have no eccentricities, and when their mutual inclination is supposed to be definitely small. He made the solution of the differential equations depend on the solution of a single linear differential equation of the second order which is of a very simple form. This equation is equivalent to an infinite number of algebraical linear equations, and Hill showed how to develop the infinite determinant corresponding to these equations in a series of powers and products of the small quantities forming their coefficients. Adams in his unpublished investigations had discovered the same infinite determinant, and was thus in a position to immediately recognize the value of Hill's work. This same year (1877) Adams communicated to the British Association at Plymouth the results of a calculation of Bernoulli's numbers. Bernoulli's numbers are the coefficients of $x^n/n!$ in the expansion of $x/(1-e^{-x})$. Now $\dfrac{x}{1-e^{-x}} = 1+\dfrac{1}{2}x+B_1\dfrac{x^2}{2!}-B_2\dfrac{x^4}{4!}+B_3\dfrac{x^6}{6!}-\ldots$ in which $B_1=\dfrac{1}{6}$, $B_2=\dfrac{1}{30}$, ... $B_9=\dfrac{43867}{798}$.... The first fifteen $B$'s were calculated by Euler, the next 16 by Rothe; and in this communication Adams supplied the following 31 numbers. The difficulty of this calculation may be judged from the facts that the denominator of $B_{32}$ is 510 and the numerator is a

number of 42 figures. By means of these numbers and calculations which Adams made of the logs. of 2, 3, 5 and 7 to 263 places, he made a calculation of Euler's constant 0.577215 to 263 places. He also made a calculation of the modulus of the common logarithms to the same number of places. Mr. Shanks had previously calculated the above logarithms and the modulus of the common logarithms to 205 places, and Euler's constant to 110 places of decimals.

In 1881 on Airy's retirement from the Royal Observatory, the appointment was offered to Adams, but he declined it. He was not a business man, and probably already felt the effects of age. In 1884 he visited America, coming as a delegate to the International Prime Meridian Conference held at Washington. He also took part in the British Association meeting at Montreal, and the American Association meeting in Philadelphia. In 1889 he was afflicted by a severe illness, and after two further attacks he died on the 21st of January, 1892, in the 73d year of his age. He was buried in the Cambridge cemetery, which is not far from the Observatory. A medallion of Adams has been placed in Westminster Abbey close to the grave of Newton.

A Cambridge physician who knew him well thus sketches his character: "His earnest devotion to duty, his simplicity, his perfect self-lessness, were to all who knew his life at Cambridge a perpetual lesson, more eloquent than speech. From the time of his first great discovery scientific honors were showered upon him, but they left him as they found him—modest, gentle, and sincere. Controversies raged for a time around his name, national and scientific rivalries were stirred up concerning his work and its reception, but he took no part in them, and would generously have yielded to other's claims more than his greatest contemporaries would allow to be just. With a single mind for pure knowledge he pursued his studies, here bringing a whole chaos into cosmic order, there vindicating the supremacy of a natural law beyond the imagined limits of its operation; now tracing and abolishing errors that had crept into the calculations of the acknowledged masters of his craft,

and now giving time and strength to resolving the self made difficulties of a mere beginner, and all the while with so little thought of winning recognition or applause that much of his most perfect work remained for long, or still remains, unpublished."

# SIR JOHN FREDERICK WILLIAM HERSCHEL *

(1792–1871)

John Frederick William Herschel was born on the 7th of March, 1792, at the village of Slough, near Windsor, England. His father was Sir William Herschel, a native of Hanover, Germany, who migrated in his youth to England, became an organist and choir master at Bath, at the same time as an amateur astronomer constructed powerful reflecting telescopes by means of which he discovered a new planet Uranus, and was invited by George III to become astronomer to the court at Windsor. He finally established himself in the village of Slough, in a house where there was a suitable grassplot for the erection of his celebrated large reflecting telescope. The mother of John Herschel, née Mary Baldwin, was the only daughter of a London merchant, had been a widow, and had brought to his father a moderate fortune. His father's salary as court astronomer was only £200, but he made much money from the construction of telescopes. John was their only child, and was thus the heir to considerable wealth. He received his primary education at a private school at Hitcham, Buckinghamshire, and was then sent to the great public school Eton in the neighborhood of Windsor; he remained there for a few months only, but when his mother saw him maltreated by a strange boy he was taken home and placed under the care of Mr. Rogers, a Scottish mathematician. He must have studied the classics thoroughly for at an advanced age he translated the whole of the Iliad into English hexameters. His father realized the importance of training in mathematics. At that time mathematical science had declined in England, through adulation of Newton and antipathy towards Leibnitz, but still flour-

* This Lecture was delivered on April 11, 1904.—Editors.

ished in Scotland. Herschel himself says, " In Scotland the torch of abstract science had never burnt so feebly nor decayed so far as in England; nor was a high priest of the sublimer muse ever wanting in those ancient shrines, where Gregory and Napier had paid homage to her power." At that time, a Scotsman named Ivory was almost the sole British mathematician who was in touch with the great mathematical progress being made on the Continent, especially in France. John Herschel possessed the great advantage of living in a home where the chief languages of the Continent were understood, and in which relations with abroad were still maintained.

At the age of 17 Herschel entered St. John's College, Cambridge. His principal undergraduate friends were Charles Babbage and George Peacock, and all three were impressed with the decline of mathematical science in England. Herschel thus describes the situation: " Students at our universities, fettered by no prejudices, entangled by no habits, and excited by the ardour and emulation of youth, had heard of the existence of masses of knowledge from which they were debarred by the mere accident of position. There required no more. No prestige which magnifies what is unknown, and the attraction inherent in what is forbidden, coincided in their impulse. The books were procured and read, and produced their natural effects. The brows of many a Cambridge moderator were elevated, half in ire, and half in admiration, at the unusual answers which began to appear in examination papers. Even moderators are not made of impenetrable stuff; their souls were touched, though fenced with sevenfold Jacquier, and tough bullhide of Vince and Wood. They were carried away with the stream, in short, or replaced by successors full of their newly-acquired powers. The modern analysis was adopted in its largest extent." The three undergraduates accomplished their object by forming an Analytical Society. The Society published a volume of memoirs but more important still they translated and published Lacroix's smaller *Treatise on the Differential Calculus*, to which Herschel added an appendix on Finite Differences.

While undergraduates both Babbage and Herschel attended the lectures of the professor of Chemistry, they helped the professor to prepare his experiments, and they set up private laboratories for themselves. Herschel finished his undergraduate career in 1813 by being a senior wrangler; he also won the first Smith's prize. He was immediately elected to a fellowship in his college. While an undergraduate he wrote a paper on "A remarkable application of Cotes' Theorem," which was published in the *Transactions* of the Royal Society, and he had no sooner graduated than he was elected a Fellow of that Society. It was his father's desire that he should enter the church, but he himself preferred the profession of the law; so in 1814 he was entered as a student of Lincoln's Inn, London. Residence in the metropolis brought him into intimate relations with the principal scientists of the day; among whom was Wollaston, the physicist (who was the first to notice two or three of the most conspicuous dark lines of the solar spectrum) and South, the astronomer. By Wollaston he was influenced to take up chemistry and optics, and by South to turn his attention to the unfinished researches of his father. The professor of chemistry at Cambridge whom he had assisted was killed accidentally; Herschel applied for the chair, but unsuccessfully. After two years spent in London he returned to Slough with the definite purpose of taking up astronomical research. To this step lines written by himself doubtless refer:

> To thee, fair Science, long and early loved,
> Hath been of old my open homage paid;
> Nor false, nor recreant have I ever proved,
> Nor grudged the gift upon thy altar laid.
> And if from thy clear path my foot have strayed,
> Truant awhile, 'twas but to turn, with warm
> And cheerful haste; while thou dids't not upbraid,
> Nor change thy guise, nor veil thy beauteous form,
> But welcomedst back my heart with every wonted charm.

During the six following years he worked at pure mathematics, astronomy, experimental optics and chemistry. It was in these years that he made his principal contributions to

pure mathematics. Several of the papers which he contributed to the Royal Society dealt with the calculus of finite differences; for these he received the Copley medal in 1821. In astronomy, he revised the catalogue of double stars made by his father; this work he did in conjunction with (Sir James) South and with the help of two refracting telescopes the property of that scientist. The resulting catalogue, printed in the *Philosophical Transactions*, brought its author the gold medal of the recently instituted Astronomical Society of London; also the Lalande prize for astronomy (of the Paris Academy) for 1825. Herschel along with Babbage took an active part in the foundation of the Royal Astronomical Society; he wrote its inaugural address, and was its first foreign secretary, while his father was its first president. In optics he investigated the absorption of light by colored media and the action of crystals upon polarised light. In chemistry (1819, when philosophical chemistry was perhaps at its lowest ebb in England) he rediscovered the hyposulphite salts, and ascertained their leading properties, the principal of which is dissolving the nitrate of silver—a property applied by Daguerre twenty years later to fixing photographic pictures. In 1821 he traveled in Italy and Switzerland with Babbage.

In 1822 his father died. His mother continued to reside at Slough, and the younger Herschel now succeeded to all the property, astronomical and otherwise, of his father. His mother survived for ten years, and throughout this interval Herschel made his home at Slough, with the exception that for three years, 1824–7, while he was secretary of the Royal Society he had also a house in London. Towards the end of this interval he married, the object of his choice being Margaret Brodie Stewart, the daughter of a clergyman of the north of Scotland; in this as in many other matters Herschel was a fortunate man. In 1830 he was put forward as the scientific candidate for the presidency of the Royal Society, the titled candidate being the royal Duke of Sussex; in which contest rank prevailed, but the principle which Herschel stood for ultimately prevailed. In this interval he accomplished much work in astronomy. In

1825 he received from his aunt, Caroline Herschel, a copy of her zone catalogue of nebulæ; in his reply he said, "Those curious objects I shall now take into my especial charge; nobody else can see them." He referred to his being the owner of a 20-foot "front view" reflector constructed by himself with his father's aid in 1820. With this instrument he made a great review of all the nebulæ visible in England, the result being a catalogue of 2307 nebulæ, of which 525 were discovered by himself; presented to the Royal Society in 1833. Herschel also continued the search for double stars, using the larger telescope which belonged to South; he discovered 3346 pairs, and made extensive measurements of known pairs.

For Lardner's Cabinet Cyclopedia he prepared an article on astronomy which was subsequently rewritten and published in 1849 as a book under the title *Outlines of Astronomy.* This book went through many editions, and was translated into many languages, even the Roman, Chinese and Arabic. For this Cyclopedia Herschel also prepared an introductory volume under the title *Preliminary Discourse on the Study of Natural Philosophy.* By Natural Philosophy he does not mean Physics only but it includes the experimental and observational sciences, namely, in the order of Herschel's book, Mechanics, Optics, Astronomy, Geology, Mineralogy, Chemistry, Heat, Electricity, Zoology, Botany. Herschel advanced several of these sciences, and had a special knowledge of all, excepting perhaps the two last; he was thus rarely well fitted to write on their logic and methods. The work treats of the methods of scientific research since the time of Francis Bacon. On the title page is a picture of Bacon and the words *Naturæ minister et interpres* taken from his first aphorism; (these words, as all in this audience know, are also in the motto of Lehigh University). In it will be found many of the philosophic ideas which were elaborated by the British mathematicians whose lives we have discussed. Here we find the idea, afterwards elaborated by Clerk Maxwell, that the atoms of the chemist bear the characters of "manufactured articles"; here we find the thought, elaborated by Tait in verse, that Nature presents to us in a confused and

interwoven mass the elements of all our knowledge and that it is the business of the philosopher to disentangle, to arrange, and to present them in a separate and distinct state. In the works of these great scientists there is abundant evidence that this *Discourse* formed a guide and inspiration, as indeed it did to all the British scientists of the nineteenth century. The *Discourse* was translated into French, German and Italian, and was reprinted in 1851.

After his mother's death Herschel prepared to carry out a long cherished project—a survey of the heavens in the southern hemisphere. The Government offered him a free passage in a ship-of-war; he preferred to pay his own way. On the 13th of November, 1833, he set sail with his family and instruments for the Cape of Good Hope, and arrived in the course of two months. He secured a house at Feldhausen, six miles from Cape Town, and there he erected his 20-foot reflector and 7-foot refractor, and applied them to the double stars and nebulæ. He constructed a scale of brightness by fixing the relative brightness of nearly 500 stars, using for this purpose "the method of sequences." He made comparisons not only at the Cape, but on the voyage out and back. With an actinometer of his own invention he made the first satisfactory measures of direct solar radiation.

While Herschel was busy at the Cape, an article appeared day by day in the *New York Sun* pretending to give an account of some great astronomical discoveries he had made. It announced that he had discovered men, animals, etc., in the Moon, and gave much detail. The paper by this enterprise, increased its circulation five fold, and secured a permanent footing. The article printed separately had a large sale, and was translated into various languages. The author was R. A. Locke, the editor of the newspaper; but De Morgan thought it had been written by a professional astronomer.

While engaged with the stars, Herschel had also time to help the development of the educational system of the colony. He was instrumental in initiating an excellent system of national education. Consulted on the course of study for a South

African College he gave his views in a letter which stated that too much time was given to the classical languages in the great English schools; that he attached great importance to all those branches of practical and theoretical knowledge whose possession goes to constitute an idea of a well-informed gentleman, namely, knowledge of the actual system and the laws of nature, both physical and moral; that in a free country it is important for every man to be trained in political economy and jurisprudence; that mathematics is the best training in reasoning, provided that it is supplemented with the inductive philosophy. He concluded, "Let your College have the glory—for glory it will be—to have given a new impulse to public instruction by placing the *Novum Organum* for the first time in the hands of young men educating for active life, as a textbook, and as a regular part of their College course."

After four years of work at the Cape Herschel returned to England, arriving in the middle of March, 1838. A great banquet was given him by his scientific contemporaries to which Hamilton came expressly from Dublin. Many honors came to Herschel; he had been knighted in 1831 and now he was made a baronet by Queen Victoria, on the occasion of her coronation (June, 1839); and from Oxford University, as one of the lions of the day he received the degree of D.C.L. In 1840 he removed his residence from Slough to the country house of Collingwood, near the village of Hawkhurst, in the County of Kent; andthis remained almost without interruption the scene of his future labors. For eight years his principal work was the reduction of the results of his four years of observation at the Cape. From this retreat he was called forth one year to address the students of Marischel College, Aberdeen, as their lord rector. In the ancient universities the rector was the chosen head of the student body; in the Scottish Universities the office survives in an altered form. The rector is elected by the students, usually on political grounds, and his principal duty is to deliver an address at the beginning of his term of office. The leading politicians of the day were candidates for the honor. Occasionally as in the case of Herschel,

Carlyle, Carnegie, the choice of the students is guided by other than political reasons. In 1843 Herschel made a reproduction of an engraving of the Slough 40-foot reflector which was the first example of a photograph on glass. He was the first person to use the terms *positive* and *negative* for photographic reproductions. His discovery in 1845 of the "*epipolic*" dispersion of light produced by sulphate of quinine and some other substances led the way to Stokes' explanation of the phenomena of fluorescence.

In 1845 Herschel was called on to preside at the second Cambridge meeting of the British Association. Since his own student days, Cambridge had made great progress in mathematical science. The "*d*-ists" had long since triumphed over the *dot*-ards. The Cambridge Philosophical Society had been founded for the reading and publication of scientific memoirs; the Cambridge Mathematical Journal had been founded; and the University Observatory had been made an up-to-date institution. His immediate predecessor in the chair was another "*d*-ist", George Peacock, now dean of Ely; and after the close of the meeting Herschel and Hamilton were guests at the deanery, on which occasion both essayed their poetic power. Two years before the Quaternion theory had been published, and Herschel referred to it in his presidential address. The closing passage of this address is characteristic of the man: "In these our annual meetings, to which every corner of Britain—almost every nation in Europe—sends forth as its representative some distinguished cultivator of some separate branch of knowledge; where I would ask, in so vast a variety of pursuits which seem to have hardly anything in common, are we to look for that acknowledged source of delight which draws us together, and inspires us with a sense of unity? That astronomers should congregate to talk of stars and planets—chemists of atoms—geologists of strata—is natural enough; but what is there of *equal* mutual interest, equally connected with and *equally* pervading all they are engaged upon, which causes their hearts to burn within them for mutual communication and unbosoming? Surely, were each of us to give utterance

to all he feels, we would hear the chemist, the astronomer, the physiologist, the electrician, the botanist, the geologist, all with one accord, and each in the language of his own science declaring not only the wonderful works of God disclosed by it, but the delight which their disclosure affords him, and the privilege he feels it to be to have aided in it. This is indeed a magnificent induction—a consilience there is no refusing. It leads us to look around, through the long vista of time, with chastened but confident assurance that science has still other and nobler work to do than any she has yet attempted; work which, before she is prepared to attempt, the minds of men must be prepared to *receive* the attempt; prepared, I mean, by an entire conviction of the wisdom of her views, the purity of her objects, and the faithfulness of her disciples."

In 1846 on resigning the chair at Southampton he announced that science was about to triumph in a remarkable way by predicting the position of a new planet. The following year, 1847, the *Results* of his observations at the Cape of Good Hope were published in one large quarto volume, the expense of publication being borne by the Duke of Northumberland; there may be found an extended catalogue of southern stars and nebulæ, with elaborate drawings and discussions of their relative and variable brightness. In 1850 the office of Master of the Mint, an office which had been held by Sir Isaac Newton, was changed from a political to a scientific appointment; and Herschel was appointed. He did not break up his home, but stayed himself in London as much as was necessary. He did not like the separation from his family, and after five years resigned. While holding this office, he also accepted a place on the Cambridge University Commission. After retiring from the Mint, he lived for sixteen years longer as the Sage of Collingwood. He was ever ready to help a younger or less fortunate man of science. He had an unbounded admiration for the genius and character of Sir W. R. Hamilton; he gave him practical counsel in the preparation of the "Elements of Quaternions," and in an indirect way assisted him financially in the education of his eldest son—a very unworthy recipient as events

turned out. We have seen how he was the first to recognize the work of Adams; it is not wonderful then that he retained his great popularity to the last.

Herschel and his mathematical friends all advocated strenuously the decimalisation of the coinage; that is, to retain the pound as the standard fundamental unit of financial value, and to retain or adopt only such sub-units as were decimal parts of it; the florin is the tenth part, and the farthing nearly the 1000th part (very approximate $\frac{1}{2}$ cent). Others advocated the shilling for the fundamental unit (=quarter dollar); the latter were called Little-endians, the former Big-endians. However both Big-endians and Little-endians were downed by the non-progressive element. In 1863 a bill was introduced into Parliament to legalize the French metrical system. Herschel, while favoring decimalisation, did not approve of changing the fundamental units. He argued that the French meter was not the 10,000,000th part of a quadrant of the Earth's meridian passing through Paris, but simply the *metre des Archives*; and that its authority was precisely of the same kind as the standard yard preserved in London. He also pointed out that the inch was very nearly the 500,500,000th part of the Earth's polar axis, and argued that the polar axis was a better natural unit than an arbitrarily chosen meridian. These arguments are the source of inspiration of Rankine's song about the Three-foot Rule, sang at the British Association. This is the point of contest at the present day both in America and Great Britain; it is not decimalisation but the choice of the fundamental units. The opposition comes from those who do not understand that the whole system of scientific arithmetical calculation for instance in electrical engineering, depends on the choice of the fundamental units; and that whatever the advantages or disadvantages of the fundamental French units, whole systems of derived units have been established upon them, and adopted by international conferences.

Sir John Herschel died at Collingwood on the 11th of May, 1871, in the 80th year of his age. The greatest tribute, in my opinion, to his character is the fact that amid the animosities

and feuds which troubled the lives and impaired the usefulness of many of the mathematicians of the earlier part of the nineteenth century, Herschel succeeded in retaining the love of all; he was equally the friend of South and Airy, of Babbage and Whewell. His home at Collingwood was the ideal home not of a selfish bachelor wedded to science, but of a devoted husband and loving father. "He never lost his taste for simple amusements; was in his element with children; loved gardening, and took an interest in all technical arts." His family consisted of three sons and nine daughters. His sons have continued, though not in so brilliant a manner, the scientific reputation of the Herschel family. He was buried in Westminster Abbey near the grave of Sir Isaac Newton. On his monument there is his motto *Coelis Exploratis* and a reference to Psalms CXLV, 4, 5.

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